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THE
BRITISH JOURNAL
OF
PSYCHOLOGY

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THE BRITISH
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INSTINCT AND THE UNCONSCIOUS.

I.

BY W. H. R. RIVERS.

IT is necessary to begin by making as clear as possible the sense in which the two terms of the title of this symposium should be used. In my contribution to the Symposium of 1918 on "Why is the 'Unconscious' unconscious?" I stated the sense in which I used and shall continue to use the term 'unconscious.' I use it for experience which is not accessible to consciousness except under certain special conditions and yet is capable of influencing consciousness and conduct indirectly in various ways. It is now necessary to undertake the far more difficult task of defining 'instinct.'

At one time, not very long ago, it was sufficient to regard instinct as the mode of mental activity proper to animals in distinction from intelligence which was held to be the attribute of the human mind. Increasing knowledge, such as that set forth in the Symposium of 1909 on "Instinct and Intelligence" and that gained through all recent work on animal psychology, has shown the distinction to have little value. It is now generally recognised that the behaviour of animals shows many features, such as adaptability to unusual conditions, which can only be explained by qualities of the same order as those belonging to intelligence. The behaviour of animals does not differ from that of Man in kind, but rather in the relative degree and importance of the different reactions of which the behaviour consists.

Another widely held distinction between instinct and intelligence is that the former is innate and the latter acquired. If an animal behaves

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

in a certain way which is quite independent of any experience it can have acquired in its individual existence, so that its nature must have been wholly determined by innate disposition, the behaviour is regarded as purely instinctive, while in other modes of behaviour regarded as partly instinctive and partly intelligent, the instinctive component is distinguished by its independence of acquired experience. This distinction, if adhered to strictly, is one of great value, but it is biological rather than psychological in nature. It is one which is very difficult to utilise scientifically, for as soon as an animal has acquired experience of any kind, it becomes a matter of the greatest difficulty to distinguish between the innate and the acquired conditions. In many cases, as that of the insect or other kind of animal acting in a highly complex way of which it cannot possibly have acquired any individual experience, the distinction is valid and useful, but in the case of Man and other higher animals, it is of little value in the attempt to distinguish between the different modes of mental activity.

A third mode of distinguishing between instinct and intelligence is psychologically even less valid than the last. In the higher animals, *i.e.* in those which have developed a cerebral cortex or neo-pallium as part of their central nervous system, instinct is regarded as the product of subcortical activity, while intelligence is held to depend on the activity of the cortex or neo-pallium. It is instructive to observe that in the last resort even so psychological a writer as Lloyd Morgan is continually driven to employ this difference in his work on instinct and intelligence[1], thus virtually giving up the attempt to make any psychological distinction between the two.

To the philosopher and psychologist, such means of distinguishing instinct from intelligence as those which have been mentioned can hardly be satisfactory. The psychologist especially is compelled by the canon of his study to look for some distinction capable of expression in psychological terms, for something by which the two kinds of behaviour can be distinguished quite apart from their exhibition by animal or Man, from their dependence on, or independence of, acquired experience, and from the part of the nervous system with which they are associated. The psychologist requires something in the nature of the behaviour itself by means of which he can distinguish the instinctive from the intelligent.

In seeking such a distinction it will be well to turn away from the behaviour of insects and other invertebrata which are usually taken as the model for the instinctive. These animals differ so enormously from ourselves that it is too great an adventure into the unknown to base any distinction on the differences between their behaviour and that of Man. Let us rather look to the behaviour of Man as compared with the animals to which he is more closely related, and that of adult Man as compared with the infant, for our clue to the nature of the difference between intelligence and instinct. I have space here only to consider one such difference. An animal or child exposed to danger, which is so recognised as danger that it produces a reaction, gives itself to the reaction fully. If it runs away, it runs with every particle of the energy it is capable of putting forth; if it screams, it does so with all the vigour at its command. Its reaction by flight or scream is the same whether the danger be small or great. There is no discrimination of the degree of danger, and consequently there is a complete absence of the graduation of the reaction to the nature of the stimulus which is characteristic of the animal in its more intelligent behaviour and of adult Man even when danger threatens. If the danger is sufficiently great, or if certain lines of reaction which are of relatively late development are frustrated, even the adult man will fail to graduate his reactions and will devote every atom of his available energy to flight or other form of primitive or instinctive behaviour. Thus, if he becomes angry and assumes an aggressive attitude, his anger and aggression will go far beyond those called for by the needs of the stimulus. It will be noted here that I accept the position, now very generally adopted, that such emotions as fear and anger, with the special forms of behaviour they accompany, belong to the domain of instinct.

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[1] *Instinct and Experience*, London, 1912, pp. 93, 104, 105, 107, 110, 112, etc.

Such instinctive reactions as those I have described are characterized by the absence of graduation of the reaction according to the nature of the stimulus. If they take place at all, they take place with their full strength. This form of reaction is one which is known in physiology, where it is characteristic, for example, of the behaviour of the individual nerve fibre, as the 'all-or-none reaction[1]. I propose to adopt this term for the corresponding kind of reaction in response to such conditions as danger. I suggest that this 'all-or-none' reaction is one of the properties of instinct.

It is noteworthy that the work of Head and his colleagues[2] has shown that this 'all-or-none' reaction characterizes the activity of the protopathic constituents of the afferent nervous system and of the thalamic region of the brain. If, as we suppose, this activity depends on the release of an ancient mode of reaction from an influence by which in later development it has come to be controlled, we have in this work a case in which an experimental procedure in Man has brought to light an instinctive mode of reaction, which has been superseded, at any rate in part, by a later mechanism for discrimination and graduation of response.

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[1] E D. Adrian, *Journ. of Physiol.* 1912, XLV. 389; *ibid* 1914, XLVII 460; *Brain*, 1918, XLI 26.

[2] W. H R. Rivers and Henry Head, *Brain*, 1908, XXXI. 428; Henry Head, *ibid.* 1918, XLI 67–69, 79.

According to the view here put forward the protopathic constituent of the nervous system shows characters which only appear in the behaviour of the normal man or animal when exposed to conditions which bring instinctive reactions into activity, in the cases I have taken for my examples, conditions of danger which arouse the fundamental instinct of self-preservation.

The work of Head has also shown that the sense in which it is here proposed to use the term 'instinct' corresponds in essence with that of such a writer as Lloyd Morgan. Head, especially in his work with Gordon Holmes[1], has shown that the earlier and cruder forms of central sensory function are associated with the activity of the optic thalamus, and especially of its essential nucleus. [The view here put forward therefore supports the position held by Lloyd Morgan that instinct is a function of the sub-cortical centres./

I have only dealt with one psychological characteristic of instinct, viz., that it is subject to the 'all-or-none' principle. It must suffice here to suggest that the work of Head gives the clue to other psychological properties of instinct, such as the crudeness, the vagueness of spatial reference, and the immediacy and uncontrolled character of the response, which are shown by the protopathic form of cutaneous sensibility.

The way in which it is here proposed to speak of instinct differs so widely from that in current use that the question must be raised whether it would not be wise to give up the use of this term in scientific literature and, in place of 'instinct' and 'intelligence,' to adopt Head's terms 'protopathic' and 'epicritic.' These words, used originally to indicate the primary nature of one kind of sensory process and the later and discriminative character of the other, are equally appropriate to indicate the distinction between the earlier and later forms of the more general reaction.

I can hardly leave this attempt to define the nature of instinct psychologically without a reference to the reactions of insects from which our notions of instinct are so largely drawn. It is quite clear that the

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[1] *Brain*, 1911–12, xxxiv, 102,

'all-or-none' principle does not hold good of the activity of the bee when constructing the cells of the honey-comb nor of the cruder art of such an animal as the grub of the Capricorn[1] (*Cerambyx miles*) in its elaborate preparations of conditions which will allow the developed beetle to emerge from its larval home.

The 'all-or-none' principle seems to hold good of many reactions of the insect, but it is evident that some other principle must also be in action, giving to the behaviour of the insect the power of discrimination and graduation of response. The lines on which development has taken place are probably very different from those which have been followed in the case of the vertebrata. The vast difference between the central nervous system of an insect and that of a vertebrate animal would lead us to expect a corresponding difference in the nature of the controlling and graduating mechanisms of the two kinds of animal. If the view here put forward seems worthy of adoption as a working hypothesis, it would become the duty of the student of insect behaviour to discover in what respects the controlling and graduating processes of the insect differ from those of Man or other higher vertebrate.

Having now made clear the sense in which I propose to use the term 'instinct,' I can proceed to state the main thesis of this opening paper of our Symposium. This thesis is that the early forms of 'all-or-none' reaction, together with the experience associated with them, are incompatible with the graduated reactions which develop later, and are dealt with by utilising a process which, as I put forward in the Symposium of last year, has always been closely associated with instinct. I refer to the process of suppression or dissociation by which the disturbing experience is not abolished, but becomes separated from the mass of conscious experience which is readily capable of recall. The view I put forward is that this suppressed or dissociated experience makes up 'the unconscious' in the sense which I have given to the term at the beginning of this paper.

I must be content here with this statement of my position, the full development of which will be undertaken elsewhere. It remains to point out that the view I have put forward implies a radical difference between the two kinds of reaction with which I deal. Instinctive reactions with their associated experience are thrust into the unconscious because they differ so greatly in nature from those developed later that the two are incompatible with one another and can only be dealt with by the drastic method of suppression. If we consider the development of mind in the light of the view here put forward, there is suggested a history in which mental development proceeded for a time along the path of the 'all-or-none' reaction. When this path had led the animal kingdom a certain distance in the line of progress, a new development began on different lines. The view I put forward is that Nature did not proceed simply by modifying the earlier process, by graduating its reactions to the needs which the animal had to meet, but started on a new path, developing a new mechanism which utilised such portions of the old as suited its purpose and treated the rest by the process of suppression. In this process those parts of the older form of reaction which were useless or noxious were shut off or dissociated from the newly developed forms of conscious reaction, only to emerge in sleep or in such states as hypnotism in which the later-developed and controlling influences are in abeyance, or have been replaced by influences of another kind. The suppression, however, is rarely complete and is always liable to break down under excessive shocks or strains. The fears or phobias and many other, more or less morbid, features of the mental life of Man are the expression in indirect and veiled form of this incompleteness of suppression, while the functional nervous disorders and insanities which occur under the stress of adverse circumstances are due to the weakening of the controlling forces and consequent emergence into activity of instinctive reactions which in health are suppressed. According to the thesis of this paper, the mental life of man is a life-long struggle between two widely differing and often incompatible modes of mental activity in which, so long as health is maintained, victory lies with one of the two opposing forces, but the vanquished force is ever there ready to reassert itself whenever the control of the victor is weakened or removed.

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[1] J. H. Fabre, *The Wonders of Instinct*, London, 1918, p. 49.

POSTSCRIPT[1].

In this opening paper of the Symposium, written six months ago, I have regarded the 'all-or-none' principle and the absence of graduation as the distinguishing marks of instinct. This conclusion was derived from the study of the instincts of Man, and especially from those varieties of instinctive behaviour which are associated with such primary emotions as fear and anger. A wider study of the subject since the paper was written has led me to take up a somewhat different position. I am now inclined to continue the use of the word 'instinct' as a term for innate mental process, and to distinguish different varieties of instinct according as they are or are not subject to the 'all-or-none' principle. If these two main varieties of instinct are denoted by means of the words devised by Head, the instincts connected with the needs of the individual will be in the main of the protopathic kind while those subserving the welfare of the group will be in the main of the epicritic kind. I now believe that the instincts which I had especially in mind when the paper was written are characterized by the 'all-or-none' principle because they came into being to meet circumstances of a primitive and urgent kind which made immediacy and fulness of response more necessary than delicacy of adjustment of means to ends. I suppose that with the gregarious mode of life there came the need for adjustment of the needs of the individual to those of the group, and that this took a large, if not predominant, part in bringing about discrimination of the conditions which call for a response and a corresponding graduation of the response itself.

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[1] This postscript had not been added when the preceding paper was submitted to the other contributors to the Symposium.

INSTINCT AND THE UNCONSCIOUS[1].

II.

By CHARLES S. MYERS.

An important initial aim of Dr Rivers's paper is to find a satisfactory psychological definition of instinct, and he bases it on the 'all-or-none' principle. To me this result is of special interest, as it recalls an earlier Symposium, on the nature of sensation intensities, in my contribution to which[2] I endeavoured, mainly on the results of Sherrington's researches, to apply the same principle to a differentiation of various classes of sensation and of reflex action. On that occasion I pointed out that three classes of reflex action are easily recognisable. There is (i) the simple 'all-or-none' reaction, illustrated by the extensor thrust reflex which is obtainable by pressing the skin beneath the pads of a dog's hind foot and is practically unaltered by varying, within certain wide limits, the strength of the ingoing stimulus. The same 'all-or-none' principle has been found in the striated muscle of the frog, and there can be little doubt that it holds for medullated nerve fibres also. "Their increase in *intensity* of function seems to depend on a greater *quantity* of elements (muscle fibres or nerve fibres) taking part in the action....Each element follows the 'all-or-none' principle[3]," although different elements may be differently sensitive, needing a weaker or stronger stimulus to excite them. In this sense only is any grading possible. If, therefore, we can imagine groups of such 'all-or-none'-reacting reflex elements, of varying sensitivity, gathered together, we reach the second type of reflex action, (ii) the 'graded monophasic' reaction, which I illustrated by the scratch reflex, where the strength of the reflex movements can be graded in amplitude, force and frequency, according to the intensity of the stimulus. This I distinguished from (iii) the 'graded diphasic' reaction, where two simple diametrically opposite reflexes (*e.g.* those of flexion and extension of a joint) are so integrated that the activity of the pair is balanced, as it were, on a knife edge, in a state of continuous tone or (else) posture,—"a condition which may be described as a state of active equilibrium of the double reflex." From this state the activity of the reflex "may be made to swing in one or other of two opposite directions as between two poles," and a condition of temporary equilibrium or adaptation may be set up within wide limits of such swing. The "afferent impulses which cause (or are set up by) reflex *contraction* of a group of muscles governed by one centre of the double reflex simultaneously cause *inhibition* of contraction in the antagonistic group of muscles governed by the other centre of the same reflex. It is the special rôle of such reciprocal graded inhibition to procure an exact adjustment between the strengths of incoming stimuli and outgoing discharge[1]."

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

[2] This *Journal*, 1913, VI. 137–154.

[3] *Ibid.* 143.

The 'all-or-none' and 'graded monophasic' reflexes are characteristic of the spinal cord; each reflex element is easily fatigable and shows a well-marked 'refractory period,' during which the movements produced by it are inexcitable. The full-blown 'graded diphasic' reflex, on the other hand, occurs in the decerebrate animal, in which the spinal cord retains its connexion with the bulb, pons and mid-brain; it is devoid of refractory period: its activity is continuous and, within certain limits, it is indefatigable. The outgoing impulses, instead of being periodically cut short, outlast the stimulus, so as to produce continuous, instead of intermittent, reflex muscular contraction. Moreover, prolonged excitation of one member of the double reflex, accompanied by prolonged inhibition of the other member, tends to give way to a reversal of these processes: the balance, when disturbed, swings sooner or later in the opposite direction.

It is not difficult to imagine that far more complex groupings of reflexes occur in higher regions of the central nervous system, giving rise, for example, to what may be described as (iv) the 'graded polyphasic' reaction, where one complex reflex action may be followed by one or other of a variety of complex, not necessarily wholly antagonistic, reactions, according to the condition of the centres concerned.

My object in such classification of the reflexes was to point out (*a*) that the 'heat and cold spot' systems of cutaneous sensibility bear a striking analogy to the first class of reflex actions, in respect of the 'all-or-none' principle, the absence of adaptation, the easy fatigability of the spots and the different degrees of sensitivity of different spots; (*b*) that in the 'warmth and cool' system of cutaneous sensibility (and in the paired 'blue and yellow,' 'red and green' systems of retinal sensibility) we can find an equally striking analogy with the third class of reflex actions, in which gradation, adaptation and (in vision) reversal are such characteristic features; (*c*) that auditory sensations—where neither the fatigability and 'all-or-none' principle of (*a*) occur, nor the bi-polarity and adaptability of (*b*),—may be likened to the second class of reflexes. And it seems possible that the peculiar contrast relations and possible fusions between the taste sensations of sweet, sour and salt[1], may prove analogous to the behaviour of the fourth class of reflex actions which I have sketched in this paper.

---

[1] This *Journal*, 1913, VI. 146–7.

But Dr Rivers has now enlarged this series of attempted analogies; he has ingeniously extended it to instincts. He considers that in instincts may be recognised the 'all-or-none' principle, "the crudeness, the vagueness of spatial reference, and the immediacy and uncontrolled character of the response, which are shown by the protopathic form of cutaneous sensibility."

I propose to try to cast the net even wider. Can we usefully bring still other modes of mental activity into analogy with the various reflex and sensory systems already classified? Let us turn first to pleasure and displeasure. There can be no doubt that they are finely graded, and are hence not analogous to the first simple 'all-or-none' class. There can equally be no doubt that pleasure and displeasure must be grouped as antagonistic, incompatible, incopresentable members of a pair, that they are set as on a swinging balance, and that they are readily susceptible to equilibrium and to adaptation. Their likeness to the third or 'graded diphasic' class is therefore unmistakeable.

Now we have a considerable body of evidence that pleasure and displeasure are intimately dependent on the activity of the optic thalami. But the striking characteristic of thalamic activity is its 'protopathy.' When through injury or disease cortical control is removed, 'thalamic' affective and sensory experience becomes grossly exaggerated, diffuse, practically gradeless, with little reference to subject or object. We conclude, then, that although in the intact human organism pleasure and displeasure belong to the 'graded diphasic' class, they are nevertheless built up from the more lowly 'all-or-none' class[2].

But the optic thalami are concerned not only with protopathic sensibility and affectivity but also with the emotional experiences which Dr Rivers agrees to include in the term instinct. If, as has been just pointed out, sensory and affective experiences become graded owing to cortical control, there seems *a priori* no reason why (at least some) emotional experiences should not acquire similar gradation, and yet retain their specific character. Moreover if, as has been previously maintained, we can distinguish several classes or levels of reflex actions, there seems *a priori* no reason why different instinctive (including emotional) experiences should not belong to different classes or levels, some obeying the 'all-or-none' principle, others being graded.

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[1] Cf. my *Text-book of Experimental Psychology*, Cambridge, 1911, I. 103, 105–6.

[2] I suggested a similar complex origin of colour sensations in the Symposium on sensation intensities (*loc. cit.*).

Certain emotions, sentiments and attitudes are clearly on the same plane as the graded affections of pleasure and displeasure,—for example the antagonistic pairs of self-assertion and self-abasement, self-confidence and self-distrust, certainty and doubt, familiarity and strangeness. But Dr Rivers would probably exclude these experiences from his definition of instinct. He accepts "the position...that such emotions as fear and anger...belong to the domain of instinct"; these, however, are the only emotions which he expressly cites as characterized by complete absence of gradation.

Now it is quite obvious that fear, anger and, I would add, sexual and maternal love stand on a different plane from pleasure and displeasure and from the other pairs of antagonistic experiences just mentioned. They are far more potent and passionate, they obtain far stronger hold over the individual, they are of rarer occurrence, and they are of greater importance for the preservation of the species. They cannot be grouped in pairs of incompatible, incopresentable members; there is no single emotion exactly antagonistic to fear, anger or love; indeed in their later development, organization and combination, a variety of mental states, not necessarily antagonistic, may ensue from any one of them. They thus come to resemble the fourth or 'graded polyphasic' class of reaction.

I find it difficult to accept Dr Rivers's contention that in children and animals fear and anger obey the 'all-or-none' principle. Surely children and animals may experience mere timidity and annoyance, on the one hand; or they may suffer uncontrollable terror and anger, on the other; or any grade may occur between these extremes. Nevertheless, within certain limits I believe Dr Rivers's view to be practically correct, for it seems to me that *at their first appearance* fear and anger cannot but *tend* to follow the 'all-or-none' principle. This also occurs in the case of adolescent 'falling in love' or 'becoming religious.' At their first entry such experiences must *tend* to react 'for all they are worth.' If one could assume that the experiences were *entirely* new, I would omit the words 'tend to.' But although no experience can ever be entirely new, so close is the approximation in these cases that the two words have hardly more than a theoretical value. Still, with the further growth and repetition of such instincts, with the increasing prominence of intellectual factors, and with the developing powers of comparison and self-control, the 'all-or-none' principle quickly gives way, these instincts become graded, and analogy to the first class of reflex actions is replaced by analogy rather to the fourth.

But, we may ask, does not every experience obey the 'all-or-none' principle on the occasion of its first occurrence? Does not the first pleasure or the first sensation of sound obey the same principle? No doubt the impossibility of determining the moments (if they exist) when such experiences *first* occur, must rob our answer of any practical importance. Yet it can hardly be ruled out of theoretical consideration, if my agreement with Dr Rivers's definition of instincts is limited practically to the first occasions of their appearance. Indeed I am disposed to think that Dr Rivers is fundamentally in agreement with me, for he writes: "as soon as an animal has acquired experience of any kind, it becomes a matter of the greatest difficulty to distinguish between the innate and the acquired conditions."

In a previous Symposium on instinct and intelligence, I gave reasons[1] for regarding their separation as a convenient but purely artificial act of 'abstraction.' I maintained that in the intact organism "there is not one nervous apparatus for instinct and another for intelligence[2]." For me, instinct wholly severed from intelligence is indistinguishable from reflex activity. Let me endeavour to justify this psychological standpoint on the grounds of further physiological parallelism.

The optic thalami, the probable seat of 'abstract' instinctive experience, never function in the intact 'individual' without some measure of cortical control. The cerebral cortex and the thalami have evolved from a common origin,—the prosencephalon, which contains within it the rudiments of later cortical and thalamic structure[3]. Only from injury or disease can the thalami act independently of the cortex. Now just as the prosencephalon has become differentiated into the optic thalami and cerebral cortex, so, I suggest, 'abstract' instinct and intelligence have evolved from a common origin, neither having a separate existence in the intact organism. Herein, I think, lies the *crux* of the difference between Dr Rivers's views and mine. He regards instinct as having "led the animal kingdom a certain distance in the line of progress," whereupon "a new development began on different lines,"—a *new path* being started, "which utilised such portions of the old as suited its purpose...." In other words, he regards intelligence as something later added to instinct, while I regard both as differentiated out of a *common origin.* His is broadly a synthetic, mine an analytic concept of mental evolution.

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[1] This *Journal*, 1910, III. 209–218, 267–270.

[2] *Ibid.* 267.

[3] Since this has been printed, I have come (mainly from correspondence with Prof. Elliot Smith) to realise that it has embryological rather than functional significance. But the general argument is unaffected: in lower vertebrates some common 'organ' is doubtless mainly responsible both for 'intelligent' and 'instinctive' activity, and these functions in the highest vertebrates have become more fully differentiated and transferred to two different 'organs,' the cerebral cortex and the optic thalami.

I am prepared to go even further. I doubt whether not only 'abstract' instinct, but also the pure 'all-or-none' principle, by which Dr Rivers seeks to define it, exists in the intact human organism. For it is only in the 'spinal' animal (*i.e.* when the cord has been severed from the brain) that the 'all-or-none' principle of the extensor thrust reaction has been purely studied in the dog. It is only under abnormal 'clinical' conditions that heat and cold spots have been completely studied and the 'all-or-none' principle of their reaction fully and clearly laid bare; and even in these circumstances the 'all-or-none' principle is only approximately obeyed. In the (intact) lower animals we have no evidence that this principle exists to the complete exclusion of graded forms of sensibility[1].

Dr Rivers is quite ready to admit that the 'all-or-none' principle does not hold for many instances of instinctive behaviour among insects. He attributes it to some unknown controlling process, distinct from intelligence. But despite the profound differences in the structure of the invertebrate and vertebrate nervous systems to which he invites our attention, I prefer to subscribe to an earlier sentence in Dr Rivers's paper,—"The behaviour of animals does not differ from that of Man in kind, but rather in the relative degree and importance of the different kinds of reaction of which the behaviour consists."

I have little space to consider whether it is correct to state that instinctive reactions are thrust into the unconscious because, being 'protopathic' in character, they are incompatible with maturer 'epicritic' experience. I do not think we have adequate evidence that in the intact organism the protopathic (p) and epicritic (e) systems of cutaneous sensibility have undergone 'dissociation,' in the sense in which the term is here used as implying the more or less independent, subconscious survival of a suppressed, non-utilised portion of experience. We find merely a composite unity $\overline{p+e}$, the constituents of which, as I have just pointed out, are only clearly differentiated and separable after neural lesion. So, I maintain, 'abstract' instinct (*ins.*) and intelligence (*int.*), though neurally differentiated, are inseparable in the intact organism; they occur as a composite unity $\overline{ins. + int.}$ It seems to me that the protopathic element is 'fused' rather than 'dissociated.'

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[1] I am confident that protopathic sensibility will not be found in a pure state on the normal human nipple and glans penis where typical epicritic sensibility is absent.

Moreover, I cannot agree that what is dissociated in any conflict between instinct and intelligence consists merely in the protopathic characters of the former, and that it is these characters that "emerge in sleep or in such states as hypnotism" or after excessive shocks and strains in which the control of intelligence is supposed to be in abeyance. Loss of control is not, I think, to be confused with protopathic reaction. The dreaming or hypnotised person exhibits graded instincts.

Whilst I agree fully with Dr Rivers that dissociation and repression occur through incompatibility and incopresentability, I would point out that the former are not the only solution for the latter. When *p* and *e*, or *ins.* and *int.* are opposed, fusion and integration, instead of dissociation and repression, may well arise from their compresence. Repression *may*, of course, occur when instinct meets intelligence, but it is not confined to such conflict. It may also occur when a lower form of instinct meets an uncompromising, incompatible, higher form, when one affective experience meets another diametrically opposite to it, or when two uncompromising, incompatible, cognitive or intellectual experiences strive independently to coexist. What is then thrust into the unconscious is not merely or necessarily the protopathic constituents, but the entire experience (cognitive, conative and affective, intellectual, emotional and 'instinctive') which is involved in the incompatible conduct. While I am writing this article, a clock strikes or some one asks me a question. I am 'not at home' to these experiences. They are temporarily inhibited or repressed, but a little later I may become aware of them[1]. Or I may forget an experience because it is unpleasant. Yet what is dissociated, what returns when control is in abeyance, is in neither case an instinct, nor is it characterized by protopathic features.

What returns from the unconscious has often suffered strange metamorphosis. I have already written at too great length to discuss the causes of incomplete repression, or the purposes of the sublimation, condensation, symbolism, etc., which dissociated experiences undergo in the unconscious. But it is clear that out of the unconscious there emerge not merely the more or less imperfectly repressed activities which have been dismissed thither through dissociation from consciousness, but also fresh activities, intellectual as well as instinctive. In the unconscious germinate (perhaps in accord with Mendelian conceptions) new instincts for the species and the creative flights of individual genius, which, when the time is ripe, sprout forth into consciousness for its use.

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[1] I am, of course, fully aware of the differences in fate that may attend an inhibited cognitive and a dissociated emotional experience.

INSTINCT AND THE UNCONSCIOUS[1].

III.

By C. G. JUNG[2].

THE subject of this Symposium concerns a problem which is important from the biological as well as from the psychological and philosophical standpoints. If we are to discuss the connexion between instinct and the unconscious, it is indispensable to use clearly defined terms. Unfortunately the terms 'instinct' and 'unconscious' belong to those more or less general conceptions, which play an important rôle in different spheres of scientific thought. While the conception of the unconscious chiefly concerns the widely separated faculties of medicine and philosophy, the conception of instinct is of equal importance in the spheres of biological, medical, psychological, philosophical and sociological thinking. On account of the wide use of the conception of instinct it is easily intelligible that it has undergone many different interpretations.

As regards the definition of instinct, I recognise that it is characterized by the 'all-or-none' reaction, as maintained by Dr Rivers, and it seems to me that this peculiarity of instinctive activity must be of great importance for the psychological side of the problem. Naturally I must confine myself to the psychological aspect of the problem, because I do not feel competent to treat the question of instinct from its biological aspect. But when I attempt to define psychologically the nature of instinctive action, I find it impossible merely to rely on Dr Rivers's criterion of the 'all-or-none' reaction for the following reason: Dr Rivers defines the 'all-or-none' reaction as a reaction that is without any gradation of intensity in respect of the circumstances which call it forth. It is a reaction that takes place with its own intensity under all conditions and without any proportion to the intensity or nature of the stimulus. But when we examine conscious processes to see whether there are any which likewise manifest an intensity which is inappropriate to the intensity of the stimulus, we easily find a great number of them in everybody: as for instance disproportionate emotions and impressions, exaggerated planning and acting, and so on. It seems to be impossible to attribute all these processes to instinct. Therefore it seems necessary to use some other criterion for our psychological definition.

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

[2] The second contribution, by Dr C. S. Myers, to this Symposium had not reached me when I wrote mine.

Colloquial language frequently makes use of the word 'instinct.' We speak of 'instinctive' actions whenever we consider an action the motive and aim of which are not fully conscious, and which has been excited by a more or less dim internal necessity. This peculiarity of instinctive activity was pointed out by Reid, who says, "By instinct, I mean a natural impulse to certain actions, without having any end in view, without deliberation and without any conception of what we do[1]." Thus instinctive action is characterized by a certain unconsciousness of the psychological motive behind it, in opposition to the strictly conscious processes which are distinguished by the conscious continuity of their motives. Instinctive activity appears to be a more or less abrupt psychic experience, a sort of intrusion into the continuity of conscious events. On account of this fact we feel the instinct as an internal necessity. In the same way Kant defined instinct as an "internal necessity[2]." On account of these qualities instinctive activity must be attributed to the realm of unconscious processes, which are accessible to conscious apperception through their results only. But were we to be content with such a definition of instinct, we should soon discover its insufficiency. When we came to examine the totality of unconscious processes, we should find it impossible to conceive of them all as instincts, although colloquial language does not differentiate them. If you suddenly meet a poisonous snake and are extremely frightened, then you rightly designate such an impulse instinctive, because there is no difference between your impulse and the instinctive fear of snakes in monkeys. It is just the uniformity and regularity of the phenomenon, which is the most characteristic peculiarity of instinctive action. As Lloyd Morgan justly remarks, it would be as uninteresting to bet on the occurrence of an instinctive reaction as on the rising of the sun to-morrow.

But on the other hand, we must not forget that psychological phenomena are not infrequently met with which are very similar to instinctive activities, although not at all to be understood as such. It may happen that some one is regularly seized with a terrible fright when meeting a harmless hen. Although the mechanism of fright in this case is an unconscious impulse similar to an instinctive impulse, we still have to draw a sharp line between the two kinds of impulse for scientific reasons. In the former case the fear of snakes is a teleological instinct of general occurrence. But the latter case is, when habitual, a 'phobia' and not an instinct, on account of its purely individual occurrence. Whereas instinct is an inherited quality, a phobia is an individual acquisition, even if it may also be the outcome of inherited disposition. There are innumerable similar unconscious necessities, for instance, thought obsessions, musical obsessions, sudden moods and phantasies, compulsive emotions and tendencies, depression and feelings of anxiety. These symptoms are met not only in abnormal persons, but also in so-called normal individuals. In so far as these phenomena occur individually only, and in so far as they are neither uniform nor regular, they must be distinguished from instinctive processes, despite the fact that their psychological mechanism is almost identical with that of instinct. They share the character of the 'all-or-none' reaction, a fact easy to observe in pathological cases. There are many such cases, where a stimulus excites a wholly disproportionate reaction, comparable to a true instinct.

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[1] *On the Active Powers of the Mind*, III. 2.

[2] *Anthropologie*, I. § 78.

But all such reactions have to be clearly distinguished from instinct. Only those processes can be called instinctive which are inherited and unconscious, uniformly and regularly occurring everywhere. At the same time, they must show the mark of compelling necessity, comparable to the character of a reflex. Instinctive activity is fundamentally distinguished from a mere sensori-motor reflex by its complicated nature only. William James calls an instinct "a mere excito-motor impulse, due to the pre-existence of a certain 'reflex-arc' in the nerve-centres[1]." Instincts have their unconscious motives, their uniformity and regularity in common with the reflexes.

The question of the origin or primordial acquisition of instinct is a most intricate one. The statement that instincts are always inherited does not explain their origin. It only puts back the problem to our ancestors. The view is widely held that instincts originated from individual and general volitional actions frequently repeated, and that they thus became automatic. This explanation is plausible only in so far as we are able to recognise how certain activities laboriously acquired have gradually become automatic through frequent practice. But if we consider some of the most marvellous instincts in certain animals, we must confess that we can hardly imagine how such instincts could have been acquired by trying, learning and repeating. There are cases where it is almost inconceivable how learning and practice could ever have taken place. Let us take as an example the instinct of propagation in *Pronuba yuccasella*, the yucca-moth. Each flower of the *Yucca* plant opens for one night only. The moth fetches the pollen from one flower, and kneads it into a little clod. Then the moth carries the pollen to another flower, where it cuts open the pistil, lays its eggs between the ovules of the plant and then climbing to the top of the pistil, stuffs the little clod of pollen into the funnel-shaped opening of the pistil. Only once in its life does the moth carry out this complicated action. If the ovary of the plant were not fertilised at the same time, the young insects could not develop; nor would the plant itself get fertilised without the help of the moth. In all such cases a possible explanation seems to be, that a moth once discovered the way by mere accident, and that its descendants became influenced during their embryonic state by the special conditions of their surroundings, so that some of them, at least, acquired an instinct to repeat their mother's accidental experience.

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[1] *Principles of Psychology*, II. 391.

But such an explanation is far from being satisfactory. Bergson's philosophy suggests another way of explanation, where the factor of 'intuition' comes in. Intuition, as a psychological function, is also an unconscious process. Just as instinct is the intrusion of an unconsciously motivated impulse into conscious action, so intuition is the intrusion of an unconscious content of an 'image' into conscious apperception. Intuition is a process of unconscious perception, either of subjective unconscious contents, or of objective but subliminal facts. Thus colloquial language speaks of intuition as instinctive apprehension (*Erfassung*). The mechanism of intuition is analogous to that of instinct, with this difference that whereas instinct means a teleological impulse towards a highly complicated action, intuition means an unconscious teleological apprehension of a highly complicated situation. In a way intuition is a counterpart of instinct, not more and not less incomprehensible and astounding than instinct itself. But we must never forget that things we call complicated or even miraculous are only so for our human mind, whereas for nature they are just simple and by no means miraculous. We always have a tendency to project into things the difficulties of our understanding and to call them complicated, while they are very simple in reality and do not partake of our intellectual difficulties. Intellect is not always an apt instrument; it is only one of several faculties of the human mind.

A discussion of the instinct problem without considering the conception of the unconscious would be incomplete, because instinctive reaction cannot be conceived unless it is understood as a psychic but unconscious process. Thus the conception of the unconscious becomes an integral part of the instinct problem.

(I define the unconscious as the totality of all psychic phenomena that lack the quality of consciousness. Instead of being called unconscious those phenomena may equally well be called 'subliminal.' The term 'subliminal' presupposes the hypothesis that each psychic content must possess a certain energic value in order that it may become conscious.) In proportion as the energic value of a conscious content decreases, the more easily the content disappears below the threshold of consciousness. From this it follows that the unconscious is the receptacle of all lost memories and of all contents as yet too feeble to become conscious. New products originate from the association and combination of unconscious contents; dreams are the commonest instances of such products. In addition to the lost memories, and the combinations not yet conscious, intentional repressions of painful and incompatible thoughts and feelings form an important part of the unconscious. I designate the totality of the contents just mentioned as the 'personal unconscious.' It contains the acquisitions of the individual life, in opposition to another stratum or form of the unconscious, containing the 'supra-individual' qualities which were not acquired but inherited, as for instance instincts, *i.e.* impulses to actions without conscious motivation. Moreover in this stratum we discover the pre-existent forms of apprehension, or the congenital conditions of intuition, viz., the 'archetypes' of apperception, which are the *a priori* determining constituents of all experience. Just as instincts compel man to a conduct of life which is specifically human, so the archetypes or categories *a priori* coerce intuition and apprehension to forms specifically human. I propose to designate the sum of such inherited psychic qualities as instincts and archetypes of apprehension by the words the 'collective unconscious.' I call it 'collective' because it does not possess individual contents of sporadic occurrence, but qualities of uniform and general occurrence. Clearly instinctive activity is an essentially collective phenomenon of uniform and regular occurrence and has nothing to do with the individual qualities of man. The archetypes of apprehension have the same uniformity and universality as instincts, and therefore equally deserve denomination as collective phenomena.

I am convinced that from the psychological standpoint the instinct problem cannot be treated without a consideration of the archetype problem, because at bottom it is the same problem. But I find it somewhat difficult to discuss these questions as opinions about the rôle of instinct in human psychology differ widely. Thus, William James held the view that man is filled with instincts, while others have restricted the number of instincts to a very few processes only slightly different from reflexes, namely to certain more or less complicated movements, *e.g.* of suckling, certain particular motor reactions of the arms, legs and larynx, the use of the right hand, and the formation of vocal sounds. In my opinion such restriction goes too far, but it is in itself quite characteristic of human psychology. Above all we should never forget that, when discussing human instincts, we are speaking of ourselves and we therefore are doubtless prejudiced.

We are far more capable of observing and judging of instincts in animals or in primitive men than in ourselves. This is due to the fact that we are accustomed to criticize our own actions, to discriminate their presumable motives and to seek rational explanations for them. But it is not yet sufficiently proven and it is even quite unlikely that our rational explanations can stand the test. A superhuman intellect is not needed in order to see through the shallowness of certain of our arguments and to recognise the true motives, namely natural instincts, behind our rationalistic constructions. It is on account of our rational arguments and our artificial reasoning that it looks to us as if we were not actuated by instinct, but exclusively by conscious and rational motivation. Of course I do not mean to say that by careful training man has not sometimes succeeded in transforming instincts into volitional actions. There is no doubt that the instincts of civilised man have become considerably modified; but underneath, instinct remains as the motive nucleus. We have doubtless succeeded in wrapping up a great number of instincts in rational motivations and volitional purposes to such an extent that we are now unable to recognise instinct behind so many veils. Moreover, with regard to civilisation, it is perhaps not desirable to see too clearly how powerful instinct is, and how thin the stratum of civilisation is. We have reason enough to make us believe that the number and intensity of human instincts have decreased. But if we apply the criterion of the 'all-or-none' reaction to our actions, then we find numerous cases of *excessive and exaggerated* reactions. Exaggeration is a peculiarly common human manifestation, although everybody carefully tries to explain his reactions by rational motives. There is, of course, no difficulty in finding good arguments, but they never alter the fact that our reaction was exaggerated and out of proportion to the exciting cause. And why does man not do or say, give or take, just as much as is needed or reasonable or justifiable in a given situation, but so frequently much more or much less? It is because an unconscious impulse is released and carries the action sometimes far beyond the limits of logical motivation and proportion. This phenomenon is so uniform and so regular, that we can only designate it as instinct, though nobody would immediately recognise the instinctive nature of the reaction.

Thus I feel convinced that human actions are influenced by instinct to a far higher degree than is usually admitted. I also believe that the rational motivation of our actions and reactions is an explanation *a posteriori* rather than a true motivation in the sense of a rational efficient cause. Our judgment in this respect seems to be biassed by an instinctive exaggeration of the rationalistic standpoint. The instinct of rationalism serves the purpose of civilised life. It conceals the impulsive character of our actions by means of logical arguments, and, as pointed out above, it aids in this way the subjection of other instincts to the real or apparent control of consciousness.

Instincts are typical ways of action and reaction, and whenever it is a matter of uniformly and regularly repeated reactions we are witnessing instinct. It is in so far quite indifferent whether there is an association with conscious motivation or not, and it is also indifferent what the momentary individual form of the action is.

Just as it is questionable whether man possesses many instincts or only a few, so it is doubtful—and this is a problem hitherto little discussed—whether he possesses many primordial forms or archetypes of apprehension or not. We meet here with the same difficulty I have already mentioned. We are so accustomed to the use of self-evident concepts that we have become quite unaware of the extent to which these concepts are founded upon the archetypes of human apprehension. The primordial forms are concealed, like the instincts, by the extraordinary development and differentiation of our apprehension. Just as certain biological views attribute only a few instincts to man, so theories of cognition reduce the archetypes of human apprehension to relatively few and logically limited categories. Plato's philosophy, however, shows a very high valuation of the archetypes. They are held to be the metaphysical paradigms or models of the real things. The real things are nothing but imitations of the model ideas. In spite of certain variations, all immediately succeeding philosophy paid a similar regard to the archetypes. Mediaeval philosophy, beginning with St Augustine (from whom I take the term 'archetype') and ending with Malebranche and Bacon, stood on the same ground of conviction. Thus the notion of archetypes, as being natural images engraved on the mind, entered into scholasticism. These images, understood as primordial

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forms of judgment, were designated 'instincts[1].' But since Descartes and Malebranche this metaphysical concept of the archetype or idea has continually decreased in importance. It gave place to thought—a psychological factor, an internal condition of cognition, as was clearly formulated by Spinoza: "Per ideam intelligo mentis conceptum, quem mens format." Finally Kant reduced the archetypes to the limited number of the categories. Schopenhauer went still further in simplifying Kant's categories, but on the other hand, he returned in certain respects to the Platonic standpoint.

This sketch is unfortunately too summary, but nevertheless it may serve to indicate that same psychological development which, as pointed out above, has concealed the instincts under the cloak of rational motivations. Here it has transformed the archetypes of apprehension, over-valued before as metaphysical ideas, into logical categories. It is no longer easy to recognise the archetypes under this veil. But the way in which man conceives the world, is still, in spite of manifold variations in detail, as uniform and as regular as his instinctive actions. Just as we have been compelled to establish the concept of an instinct determining and regulating our conscious action, we must also have recourse to the correlated concept of a factor determining the uniformity and regularity of our apprehension. It is this factor which I term the archetype, the primordial image. The image might be suitably understood as intuition of the instinct in itself, analogous to the conception of consciousness as an internal image of our objective vital processes. Just as our conscious conception determines the form and purpose of our conscious action, so unconscious apprehension determines through the archetype the form and purpose of instinct. Just as we believe instinct to be thoroughly adapted and sometimes incredibly clever, so we have to assume that intuition to which instinct owes its existence, must be of extraordinary precision.

The criterion of the 'all-or-none' reaction, pointed out by Dr Rivers, has helped us to discover the activity of instinct everywhere in concrete human psychology. I hope that my concept of the primordial image may perform a similar service, when we try to discover the activity of intuition in practical human psychology. It is quite easy to discover intuitional activity in primitive peoples. There we constantly meet with typical images and motives which are the foundations of their mythologies.

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[1] Herbert of Cherbury says: "Instinctus naturales sunt actus facultatum illarum a quibus communes illae notitiae circa analogiam rerum internam, cuiusmodi sunt, quae circa causam, medium et finem rerum bonarum, malum, pulchrum, gratum etc. per se etiam sine discursu conformantur."

Those images are autochthonous and of relatively great uniformity, as for instance, the idea of magic power or magic substance, of spirits and their behaviour, of demons and gods and their legends. We see the perfection of those images, and at the same time their envelopment by rational forms, in the great religions of the world. The archetypes appear even in the exact sciences where they are at the root of indispensable auxiliary concepts, as of the ether, energy and the atom. In philosophy Bergson affords an example of the revival of a very old image in his conception of the 'durée créatrice,' already met with in Proclus and, in a still more primitive form, in Heracleitus.

Modern analytical psychology has constantly to deal with disturbances of conscious apprehension due to the admixture of archetypes with rational conscious processes, and this occurs in normal persons as well as in pathological states. Just as the admixture of instinctive impulses with conscious apprehension produces exaggeration and distortion of our actions, so the admixture of primordial images with conscious apprehension produces characteristic misconceptions. Such misconceptions depend upon archaic prototypes simultaneously excited. Their admixture causes too strong or too weak, or in other ways distorted, impressions, and thus leads to the construction of erratic views. Such views again excite typical instincts because of the close association of instincts and archetypes. War psychology for instance has produced innumerable examples of archaic instincts and of archaic misconceptions.

Archetypes are typical forms of apprehension; indeed, wherever we meet with uniformly and regularly recurring ways of apprehension, they are referable to archetypes.

The collective unconscious consists of the sum of the instincts and their correlates the archetypes. Just as everybody possesses instincts, so he also possesses archetypes. The most striking evidence for the existence of archetypes is seen in mental derangements characterized by an intrusion of the 'collective unconscious' into the conscious, as occurs in all paranoias and hallucinatory psychoses. Here we can easily observe the occurrence of instinctive impulses associated with mythological images. It is impossible to say which is prior,—apprehension or impulse to action. It seems to me as if both belong to the same vital activity, which we are incapable of imagining as single and therefore dissect into two essentially distinct processes.

INSTINCT AND THE UNCONSCIOUS[1].

IV.

By GRAHAM WALLAS.

I PROPOSE to discuss Dr Rivers's statement of the "main thesis" of his paper. "This thesis," he says, "is that the early forms of 'all-or-none' reaction, together with the experience associated with them, are incompatible with the graduated reactions which develop later, and are dealt with by...the process of suppression or dissociation by which the disturbing experience is not abolished, but becomes separated from the mass of conscious experience which is readily capable of recall.... Instinctive reactions with their associated experience are thrust into the unconscious."

Dr Rivers's description of "the early forms of 'all-or-none' reaction" seems to me to be admirably clear. I am not so certain what he means by "the graduated reactions which develop later." In an earlier paragraph he calls them "intelligence," and says (in a still earlier paragraph), "Let us ... look to the behaviour ... of adult Man as compared with the infant, for our clue to the nature of the difference between intelligence and instinct."

His thesis, therefore, seems to be that the normal process by which an adult man now controls and graduates his simpler instincts is by thrusting them into the unconscious. With the thesis so stated I do not agree.

I do not, of course, deny that "suppression or dissociation" takes place. But I wish to argue that it is neither the only, nor the most effective way by which civilised man gains control over his instincts. It seems to me that the best process by which the child who "gives himself fully" to fear can make himself into a reliable soldier in modern warfare should be described rather as a "bringing into full consciousness" of the psychological phenomena of fear than as a "thrusting" of them "into the unconscious." Dr Myers, in answering a letter in which I argue this, puts my point well by saying: "There have been instances in this war where psychologically acute officers have trained their men by telling them exactly what to expect [*i.e.* what feelings to expect] in the trenches, with the result that the emotions have been 'graded' or 'under control.'"

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

A kindred question arises in pedagogy—Shall we, as the 'sex-educationalists' propose, tell the child before puberty "what to expect" when puberty comes, or shall we, by compulsory football, etc., attempt to thrust the sex instinct into the unconscious?

It must be remembered that, in the case of sex, and, under the conditions of modern warfare, in the case of fear, there is no hope that the instinct will not, sooner or later, enter consciousness. The question is whether it shall enter a consciousness that expects it, and can therefore recognise it and watch it critically, or not? In the case of fear I believe that under modern conditions the method of bringing it into consciousness is better than that of ignoring or suppressing it, even when suppression is accompanied by the creation (through mechanical drill) of a strong habit of obedience to orders. The modern European soldier is much less liable to panic than his ancient or mediaeval predecessors, and that fact is apparently due in large part to his custom (which reminds one of what one is told of the Freudian methods of treating acquired phobias) of discussing freely with his fellows 'cold feet,' the 'wind up' and other physiologico-psychological phenomena of fear.

There is a sense in which this bringing of instinctive phenomena into full consciousness may be said to involve a separation of them from the 'self' or 'will.' When an ancient Greek or Persian was seized with panic, the panic was his 'self.' He, in Dr Rivers's phrase, "gave himself fully" to it. When a British soldier discusses his 'cold feet,' or I myself watch my knees trembling as I stand at the edge of a precipice in the Alps, the cold feet and the trembling are a nuisance, but are not one's 'self.'

Our consciousness of, and separation of our 'self' from our instinctive feelings, and, therefore, our power of controlling our actions in their presence, may be increased in efficiency, not only by our expecting and recognising them, but also by our 'understanding' them—that is to say by our giving them a place in our general conception of cause and effect.

Dr Rivers's argument as to the 'all-or-none' nature of instinct raises the further question whether the bringing into consciousness of an instinct weakens, or intensifies, or, as he would seem to argue, leaves unchanged its actual manifestation. In the case of some of the simpler reactions I suppose that no change takes place, though I have no experimental knowledge on the point. When one's nerves of vision are tired, the resulting changes of colour sensation may be the same to the student who expects them and to the student who does not expect them. My knees may tremble in the Alps no more or less because I 'know all about it' (though in fact my impression is that they tremble less). In the case of fear, I should suppose that the 'feet' of the soldier who expects and recognises 'cold feet' are in fact less 'cold' than the feet of the soldier to whom fear comes as a surprise unrelated to the other contents of his mind.

The general question as to the relative efficiency of thrusting into the unconscious and bringing into consciousness as methods of control can be asked in approaching a large number of the more difficult problems of civilised conduct. In civilised life, for instance, men, whether they are professors, or officials, or artisans, or clergymen, are from time to time called on, owing to some new discovery or invention, to change their methods of work. Men have a natural shrinking (which, as it arises from a typical innate disposition, should, I suppose, be called an instinct[1]) against change of habit. If a man is not looking for that shrinking, and does not 'understand' it, it is, when it comes, part of himself, and therefore he has no fulcrum of resistance to it. He simply hates the faddists, or heretics, or blacklegs who are suggesting change, without understanding or controlling his hatred. In that, as in many other respects, the full use of applied science in civilised life is waiting for a general increase of psychological self-consciousness.

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[1] It is a pity that one has to use the word 'instinct' (which means a particular kind of natural disposition) when one simply desires to say that some fact is part of our 'nature' and not of our 'nurture.' It would be very convenient if one could call any fact of our nature a 'nat,' whether it is the shape of our skull or our fear of snakes, and any fact of our nurture a 'nurt,' whether it is the scar of a wound or knowledge of Greek. One could then speak of physiconats and psychonats, and of physiconurts and psychonurts. One could say "shell shock should be treated rather as a psychonurt than as a physiconurt"; or that "an intense fear of snakes may either be a nat or a nurt; an intense fear of cats is almost certainly a nurt." But I suppose that we must wait till somebody invents more euphonious words than 'nat' and 'nurt.'

INSTINCT AND THE UNCONSCIOUS[1].

V.

BY JAMES DREVER.

DR RIVERS begins by attempting to make clear the sense in which the two terms 'instinct' and 'unconscious' are understood by him, and the sense therefore in which they ought to be understood in the present discussion. Whether the attempt is wholly successful may be doubted. At all events his definition of instinct involves a theory which does not appear to be accepted by Dr Jung, and is not wholly accepted by Dr Myers, while his view of the unconscious, carried over from the 1918 Symposium, involves a theory which is not adopted by Dr Jung. Hence it might be well to devote part of the present paper to a further survey of the ground and a further attempt to clear up the issues in the present discussion, so far as a psychological universe of discourse is concerned. This is all the more necessary owing to the fact that both terms tend to be used somewhat loosely and indefinitely, sometimes with a psychological connotation, sometimes with a physiological, sometimes with a biological, and not infrequently with a philosophical or metaphysical. Especially perhaps is this the case with the unconscious. The psychology of the unconscious has too often tended towards a somewhat crude mysticism which is to be deprecated in the interests of progressive science psychical or physiological. It is difficult to avoid the conclusion that such a tendency is partly the inevitable result of using a term like 'the unconscious' and resting content with mere exclusion in lieu of definition, and is partly the nemesis of the murderous attack on language perpetrated in our phrase 'unconscious experience.'

In his contribution to the 1918 Symposium, Dr Rivers, in place of clearly defining the unconscious, chose rather to indicate the sense in which he understood the term by an illustration, a case of claustrophobia. It is true that this indication marked off a certain definite group of phenomena as that group with which he was concerned in the Symposium in question, and at the same time suggested that as far as the human being was concerned the phenomena were more or less abnormal. But in a discussion like the present something more than the citing of individual cases seems necessary if we are to arrive anywhere, and for my part I do not see my way to accept the suggestion that the term 'unconscious' should be in the main restricted to abnormal phenomena of the dissociation order, or rather that such phenomena are necessarily abnormal. I would therefore in the first instance attempt to discover in what sense we can regard the unconscious as psychical yet not conscious in the ordinary sense, and then try to bring my result into relation with the kinds of phenomena we can agree to group under the head of 'the unconscious.'

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society and the *Mind* Association, in London, 12 July, 1919.

I take it that 'psychical' includes facts of at least two distinct kinds, those of the order of 'dispositions'[1], and those of the order of 'experiences.' Dispositions as such, though determining experience, are never experienced. They therefore necessarily belong to the sphere of the unconscious in a wide sense. This is not, however, the sense in which Dr Rivers ostensibly uses the term, for he speaks of "experiences of which we are not conscious"[2], and in the opening paragraph of his paper he explicitly states that he uses the term "for experience which is not accessible to consciousness except under certain special conditions," though it is worth noting that both he and Dr Jung actually do include dispositions among the facts of the unconscious which they cite and discuss, and it may be truly said of dispositions that they are "capable of influencing consciousness and conduct indirectly in various ways." Let us however consider experiences. What kind of experiences can we include under the unconscious? Dr Rivers specifically excludes 'marginal' experiences,—the 'fringe of consciousness'[3]. Are there experiences apart from these which the psychologist can admit as in any sense unconscious? This it appears to me is a question of great interest and importance for the pure psychologist. As a pure psychologist I am now speaking. In ordinary speech we say that we are conscious only of that of which we are *personally* conscious. I do not think there can be any doubt that there is 'subpersonal' consciousness, as for example a purely perceptual consciousness.

I have elsewhere[4] described consciousness as 'psychical integration,' and this integrating function I take to be the peculiar and unique characteristic of experience. But in addition to the psychical integration in different degrees, which is consciousness, psychical life presents a character which we may call 'aggregation,' or better 'synthesis,' and again in varying degrees. The highest degree of synthesis is presented by the rational, self-conscious psychical life. A lower degree of synthesis is exhibited on the level of ideal representation. A lower degree still is shown on the purely perceptual level. In the first case the synthesis clearly constitutes a 'self' or 'person.' In the second case this is not so clear, but there is nevertheless a continuity, which may without any great strain of language be also called personal. But the synthesis on the purely perceptual level is without doubt 'subpersonal.' It ought to be noted also that the syntheses of the psychical life are at bottom dispositional, and only indirectly, as it were, affect experience[1].

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[1] Stout, *Manual*, I. ch. ii.

[2] This *Journal*, IX. 237.

[3] *Loc. cit.*

[4] *Instinct in Man*, Cambridge, 1917.

I would suggest then that experiences which are subpersonally conscious and the corresponding dispositions would include some at any rate of the phenomena at present under discussion. Repressed or dissociated experiences or dispositions would be included, as well as experiences of which we were never personally conscious. But this 'unconscious' is not really unconscious. An unconscious which is really unconscious, apart from the disposition, does not appear to be psychical at all. The unconscious I would regard as subpersonal consciousness, including, in so far as it is experience, purely perceptual or even subperceptual experience.

The meaning of instinct for the psychologist I have discussed at length elsewhere[2], and there is no purpose to be served in repeating the discussion. Briefly, the sense in which I understand the term is 'determinate conscious impulse which is not determined by previous individual experience, but which nevertheless enters into, and determines individual experience and attitude.' Structurally such determinate conscious impulse would be represented by an innate psychophysical disposition, functionally by an experience, normally at the subpersonal level when occurring in its purest form. Regarded in this way the psychological relations of instinct and the unconscious become more or less obvious. In its wider sense, as inclusive of both disposition and experience, the unconscious covers a group of psychical facts of which the facts of instinct constitute a part. In its narrower sense as applied to experiences, which, as Dr Rivers says, are not conscious, and which I prefer to call subpersonal, the unconscious again covers a group of facts, of which the facts of instinct (including appetite) probably constitute the main part. This appears to be as far as our present line of thought will take us.

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[1] I fully recognise that this necessarily brief statement is inadequate, but another opportunity must be taken for its expansion. One of the greatest services rendered to the science of psychology itself by recent work on abnormal manifestations of the unconscious is the way in which it has secured that emphasis will henceforth be laid on the exceeding great complexity of the psychical life of Man. See also below on the 'strata of consciousness.'

[2] *Op. cit.*

Hitherto we have avoided dealing with some of the most interesting points raised by Dr Rivers, Dr Myers, and Dr Jung, in order to establish a standpoint from which the various facts may be regarded. Dr Rivers suggests that the 'all-or-none' reaction is characteristic of instinctive behaviour, and that this may be taken as a basis for the psychological definition of instinct. Dr Myers, however, shows that this kind of reaction is characteristic of a rather primitive form of reflex, and hence to define instinct by means of this characteristic is impossible. It may be noted in passing that an explanation of this apparent difficulty and difference is that external behaviour can never furnish us with an ultimate psychological criterion. Reactions that are alike as far as externals go may be psychologically very different. Pure 'behaviourism' of the extreme kind invariably and inevitably breaks down as a psychology. Nevertheless this 'all-or-none' kind of behaviour is worthy of the most careful attention. It is clear that reactions which have the highest possible specificity will inevitably be of the 'all-or-none' description. It is *this particular element* in the situation, and nothing else matters; it is *this specific response* and nothing else. Physiological implications apart, the 'all-or-none' type of reaction is preeminently characteristic of behaviour on the purely perceptual plane, though it is only when we concentrate attention on one particular reaction that this becomes evident. The grading of reaction upon which both Dr Myers and Mr Graham Wallas lay stress, is, as it were, a subsequent characterization from the point of view of a higher level. But in any case this type of reaction is always possible, even for the organism whose psychical development is far above the purely perceptual plane, whenever at the particular moment of reaction the operation of the higher psychical levels is for any reason suspended, a notable instance of which in normal life is the case of violent emotion. As McDougall has pointed out[1], the primary emotions are invariably bound up with instinctive behaviour, and it is precisely those emotions which manifest themselves most frequently in a violent form, and which therefore tend to determine action at a purely perceptual level. Hence instinctive behaviour will always in a certain sense be of the 'all-or-none' type, and this sense seems to have largely influenced Dr Rivers in his emphasizing of this type of reaction in connexion with instinct. In this sense I agree with him as against Dr Myers and Mr Graham Wallas.

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[1] *Social Psychology.*

Dr Rivers initiates a still more interesting, if somewhat speculative, line of thought by his attempt to correlate the difference between instinctive and intelligent behaviour with the difference between protopathic and epicritic sensibility, and his suggestion that we should henceforth substitute the two terms 'protopathic' and 'epicritic,' as descriptive of two psychical levels, for the terms 'instinct' and 'intelligence,' in our psychological terminology. Like Dr Myers I wish to cast the net still wider, and wider even than he does, though in a different direction. Dr Jung has distinguished between the 'personal unconscious' and the 'collective unconscious.' In the 1918 Symposium Dr Ernest Jones, following Freud, emphasized the distinction between the 'primary' and 'secondary mental systems,' which are "precursors of the unconscious and conscious mind" of adult life, the 'primary' system being "dominated entirely by the hedonic 'pleasure-pain principle,'" the 'secondary' "by the 'reality principle.'[1]" I have elsewhere[2] drawn attention to the fact that the distinction between appetite and instinct rests on a somewhat similar basis.

All this seems to indicate the desirability of recognising different 'strata' in the psychical life, one as it were overlying another. An evolutionary interpretation is almost inevitable. But due caution must be exercised in such an interpretation. We can hardly suppose that the evolution of an overlying mental system A has been unaccompanied by evolutionary changes in the overlaid system B. That is to say, we cannot assume that the system B as now existing is the primitive or primary mental system. The presumption is all the other way. This is a difficulty which is continually cropping up in the biology, no less than in the psychology of instinct. Thus, in another connexion, Dr Jung finds it impossible to explain on either the Darwinian or the Lamarckian hypothesis the evolution of such an instinctive manifestation as that of the yucca-moth. Recent biological work indeed indicates the need of supplementing both hypotheses to account for these and similar cases[3]. But, if we assume, as we should, that the relation between flower and moth has itself evolved, with evolutionary changes in flower as in moth, and indeed in the whole world to which the relation and situation belong, the matter bears an entirely different complexion. So too with regard to the overlaid system B. I am entirely in agreement with Dr Myers when he contends that the pure original protopathic sensibility is not found in the normal intact organism after the epicritic has been developed. So too the pure original instinct (in the biological sense as well as the psychological) will not be found after 'intelligence' has once functioned, at least if the intelligence be on a level higher than the perceptual.

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[1] This *Journal*, IX. 250.

[2] *Op. cit.* 255.

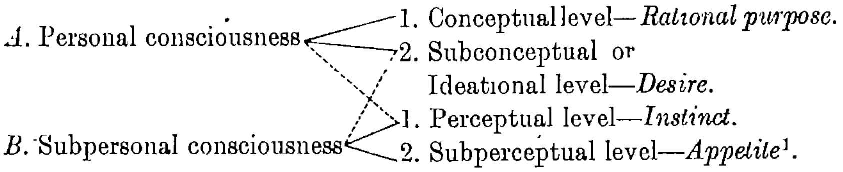
[3] Cf. Tait, "Experiments and Observations on Crustacea." *Proc. Roy. Soc. Edin.* XXXVII.

But psychologically there is a more interesting aspect of the question than that dealt with by Dr Myers. I would preface what follows by postulating that for psychology all explanation of the psychical must be based upon our knowledge of the psychical. If we abandon that principle our psychology will become capable of any extravagance. Now, since our only direct knowledge of the psychical is in our own experience, our explanation and interpretation of even the primordial psychical must be in terms of what we know of the psychical in this way. The simplest experience we know is of the perceptual order, but there is reason to suppose that experience at a lower level than our perceptual experience exists. Such experience, however, must be described and interpreted in terms which hold invariably of the experience we do know, and presumably, *mutatis mutandis*, of all experience. Hence Freud's 'primary mental system,' "dominated entirely by the hedonic 'pleasure-pain principle,'" the protopathic sensibility emphasized by Dr Rivers and Dr Myers, my 'appetite' level, must equally be regarded, —in so far as they represent a primordial, and at one stage in evolutionary history the sole, experience,—as essentially bipolar, and as essentially integrating the life activity of an organism and the nature of an environmental situation, related to the life of the organism, in the unique phenomenon we call consciousness.

If this be allowed, some interesting conclusions appear to follow. In the first place, behaviour which shows a character that may be regarded as analogous to the 'all-or-none' reflex will almost necessarily be the issue of primordial experience which is of the perceptual or subperceptual order; but in proportion as this primordial consciousness is overlaid by consciousness of a higher order than the purely perceptual, the behaviour will tend towards consciously determined coördination and grading. Moreover, with the development of discriminative sensibility, the cognitive function of the primordial experience will tend to be obscured, so that it will seem as if the affective constitutes practically the whole of the experience, and we may be led to consider this primary mental system as "dominated entirely by the hedonic 'pleasure-pain principle,'" while the secondary or higher is "dominated by the 'reality-principle,'" simply because of the obscuring of the cognitive function of the lower. But in any case this lower system I should prefer to call 'appetite' rather than 'instinct.' (There is one possibility that should not be lost sight of. That is the possibility of avenues of sensibility characteristic of the lower system being entirely obscured by the development of the higher, but becoming traceable under abnormal conditions.)

Finally, as regards the topic at present under discussion, we seem to be driven towards the same conclusion as before with regard to the unconscious and instinct. The unconscious or subpersonal consciousness will be consciousness mainly at or below the perceptual level, and therefore consciousness in which appetite and instinct will have the fullest play, but to identify the unconscious with instinct is impossible. The unconscious is wider than instinct, though undoubtedly instinct plays in the realm of subpersonal consciousness a part that is certainly more prominent, if not more important, than in the realm of personal consciousness.

I might represent the 'strata' of consciousness schematically in some such way as the following:


A. Personal consciousness
1. Conceptual level—*Rational purpose.*
2. Subconceptual or Ideational level—*Desire.*
B. Subpersonal consciousness
1. Perceptual level—*Instinct.*
2. Subperceptual level—*Appetite*[1].

How this would be related to the distinction between cortical and subcortical process on a physiological basis is not very clear, but the tentative identification of personal consciousness with cortical process, and subpersonal with subcortical is perhaps worthy of investigation.

My main conclusion as regards the subject of the present Symposium may now be stated very briefly. The unconscious or subpersonal consciousness underlies at all times the conscious or personal consciousness, just as the instinctive propensities underlie the ends and purposes of our rational activities, and it is unconscious because it represents either a stage of psychical evolution beyond which we have passed by normal development, or a mass of experience upon which we have, as it were, tried to turn our backs by some more or less abnormal process of dissociation, repression, or substitution, but instinct has precisely the same psychological position and function in subpersonal as in personal consciousness.

There is one part of Dr Jung's paper to which I should like to make some reference in conclusion. After defining the unconscious as "the totality of all psychic phenomena that lack the quality of consciousness," and after distinguishing between the personal unconscious and the collective unconscious, he suggests that the 'archetypes' of thinking[1] belong with the instincts to the collective unconscious. The archetypes are the recognised categories or forms of thought, together with unrecognised forms including forms of Bergsonian 'intuition.' While appreciating Dr Jung's general position, I do not think there is any psychological justification for thus placing instinctive processes and archetypes of human thinking side by side. Distinct modes of cognition, determinate directions of conation, specific characters of affection, are all data which the psychologist studies, and of which he attempts to give a rational description and explanation, but the forms of thought are ultimate and fundamental postulates of psychology as of all human science and philosophy. They are as ultimate as experience itself. We could not study thought as such scientifically, unless we could in some way get outside of thought; and to relegate the instincts and the unconscious to the same order of existence as the archetypes or forms of thought seems equivalent to putting both for ever beyond the range of human science.

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[1] Continuous lines show normal relationship, broken lines possible distribution

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[1] "Archetypes of human apprehension" is Dr Jung's phrase. I have interpreted this as equivalent to 'categories or forms of human thinking.' I am not sure that this is quite what Dr Jung had in mind. The *instinctus naturales* of Lord Herbert of Cherbury included both *notitiae communes* and instincts, in our sense, the former being apparently 'innate' ideas as understood by Descartes, Locke, etc. Now the "determining constituents of experience," which are *a priori*, are from my point of view either the instinctive tendencies or the categories or forms of thinking—thinking, that is, either perceptual or conceptual.

in such 'unconscious,' subconscious, or co-conscious activity. We are then discussing two distinct problems, (1) the relation of instinct to the innate structure of the mind, (2) the relation of instinct to 'unconscious' mental activity and to the mental structures involved in such activity. These two problems, though not unrelated, are distinct and require to be separately discussed.

Instinct is by general admission innate, or, as I have preferred to put it. instincts are innate dispositions, parts of the innate structure of the mind. Do the instincts of any species constitute the whole of that innate structure? In the case of the human species we may, I think, confidently answer—No, the innate structure of the human mind comprises much more than the instincts alone. I have elsewhere argued that an instinct is not only an affective or emotional-conative disposition, but also always has a cognitive side, and that the cognitive part of the total disposition which is an instinct is in many cases much more complex and highly developed than is commonly recognised; for it renders possible the perception of, and discriminative reaction upon, complex objects independently of previous individual experience[1]. If this be so then this fact in itself indicates a considerable development of the innate structure of the mind on its cognitive side. For, as I have tried to show elsewhere[2], each specialised cognitive disposition is but a differentiation of an older system[3], and its operation implies the activity of all that part of the system to which it stands in the relation of direct differentiation or evolution. But we cannot, I think, stop at this point. There are many facts which compel us to go further in the recognition of innate mental structure, such facts as the special facilities shown by individuals in music, in mathematics, in language, and other aesthetic, moral and intellectual endowments. This question of the extent and nature of the innate endowment or innate mental structure is only beginning to take shape and to be seriously considered; it remains one of the largest fields of work for psychology. I will only venture a few discursive remarks upon it. At present we are in the stage of not having secured general recognition for the instincts as part of this innate structure of the human mind; yet the instincts are the part of the innate structure which is the least difficult to discern and define. The discovery of the nature and extent of the remaining parts must be a matter of great difficulty; and for the same reason that the definition of the human instincts is difficult, namely that innate structure may be given or laid down in the mind with all degrees of definiteness and completeness, varying from the highest degree (as exemplified in some of the animals when they execute complicated and delicately adjusted perceptual reactions to objects upon their first contact with them) to a very low degree, at which the innate structure, as given, is incapable of determining the course of mental process but merely facilitates in some degree that form of mental activity or experience through which alone it becomes further elaborated and differentiated.

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[1] This, I have argued, is true not only of many animals but of man also, as most clearly in the case of the sex instinct, which is normally so far specialised on its cognitive side as to confine the specific affective response of the instinct to members of the opposite sex (cf. "The definition of the Sexual Instinct," *Proc. Roy. Soc. Med.* 1914, VII.).

[2] *Psychology, the Study of Behaviour.* Chap. III.

[3] Just as each conative part of each instinct is a specialisation of an original conative disposition through which the conative energy of the mind is directed to a special end.

If it is recognised that the instincts are but a part of the innate structure of the mind, it would yet seem that they are a very special part, and as a speculative hypothesis I would suggest that they differ from the rest not only in that they are the great channels of conative energy but also in that the nervous and bodily structures through which they operate are also innately laid down, whereas for the rest of the innate mental structure no such bodily organs are given.

In the foregoing remarks I have assumed the truth of the developmental view of mind. But it is possible to hold a 'preformation' view, to hold that what we call mental evolution in the animal world and in man is but the development of bodily and nervous mechanism through which the pre-existing powers of the soul are rendered capable of manifesting themselves progressively more fully in material organisms. Something of this sort seems to be implied in Dr Jung's doctrine of the 'collective unconscious,' and I would press him to tell us more frankly whether this is his view[1]. (On the other hand Dr Jung seems to accept a Lamarckian view of mental evolution, and to believe that the innate mental structure or 'collective unconscious' is in large part a deposit from racial experience.)

(These racial deposits he speaks of as the archetypes or primordial thought-feelings, and I gather that in his view these archetypes are in the main concerned with primitive myths and phantasies, and have little or no part in the plain thinking of the plain man in his sober waking hours. I should like Dr Jung to tell us whether our understanding of space depends upon an 'archetype' or is in any way conditioned by which become dissociated; perhaps he conceives it as Freud seems to conceive the sex instinct, namely as made up of an indefinite number of components which are shed or dissociated, during the course of individual development. If so, he should, I think, give us some indication of these components. The fact is that, before this problem of the relation of instincts to repressions, suppressions, or dissociations can be profitably discussed, it is necessary to set out clearly what we mean by instincts, what instincts we recognise in man, what is the nature, what the structure and mode of operation of each instinct that we conceive to play any such part. Dr Rivers has not attempted this (and the same complaint may be brought against Dr Jung). Presumably he does not accept as approximately correct the account of the human instincts that I have given, or he would have told us so. Implying the rejection of this scheme by his silence, he offers us nothing in its place, except the very disputable propositions that instinct "is subject to the 'all-or-none' principle" and that it has 'protopathic' properties; propositions which, even if they be true and therefore presumably useful, go but a very little way towards furnishing an alternative scheme of the instincts such as is required for any profitable discussion of the present topic.

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[1] The reader may be reminded that even so distinguished a biologist as Mr Bateson seems to find himself turning to some such 'preformation' view. (Presidential Address to Brit Assoc. 1914.)

I would suggest on the basis of my own scheme of the instincts, the following concise statement of their relation to the 'unconscious' in the second of the two senses defined above. I am not sure that we should, with Dr Rivers, identify 'dissociation' and 'suppression' or in the more usual terminology 'repression'; but, for the purpose of the present discussion, I will continue to do so. The synthetic unity of [illegible] is [illegible] tional level of mental concerned in that experience. If the cognitive disposition (or system of such dispositions—large or small) becomes dissociated from the principal system, then, since it retains its connexion with the affective disposition of the instinct (this is what is called a complex and might be alternatively called a dissociated or repressed sentiment) it is capable of functioning as an isolated system; it may then lie quiescent, or may be from time to time roused to life as a subpersonal memory manifesting its appropriate affective-conative tendencies. The instinct itself, or its affective disposition, retains its connexions with other cognitive systems, is not dissociated, and therefore continues to play its part in personal consciousness and activity.

The question to what extent such dissociation occurs in the normal person, or whether it is confined to neurotics, is too large to be raised in this discussion. But for those who have approached this problem from the clinical side, I may sum up the position I would maintain by saying that Janet is more nearly right than Freud, though Janet made the great mistake of ignoring wellnigh completely the conative factors in the production of the *amnesias* with which he has dealt in so masterly, but too intellectualistic, a fashion.

I cannot then follow Dr Rivers in the view summarised in his last paragraph. I cannot see any evidence of such discontinuity of mental evolution. The human instincts are not suppressed by new forces and incompatible modes of mental activity beneath which they crouch like a cage full of wild beasts; that is an old and popular conception of the instincts which seems to me to be wholly false. In conclusion I would ask Dr Rivers to tell [illegible]

THE RELATION OF AESTHETICS TO PSYCHOLOGY[1].

By EDWARD BULLOUGH.

1. *Subject-matter and fundamental problems of aesthetics.*
2. *Individual* vs. *social factors.*
3. *Receptive* vs. *creative aspects.*
4. *The origins of art.*
5. *Comparative aesthetics.*
6. *Conclusion.*

1. The subject-matter of Aesthetics is the 'aesthetic experience.' Its subject-matter is not Art or Beauty, because both these conceptions are, especially in their common connotations, too narrow. There is much aesthetic experience outside the experience of Art and Beauty. Let us start with the fairly safe fundamental assumption that there is such a thing as aesthetic experience. Then there are observable, in the first place, certain *objects* which we call beautiful or otherwise aesthetically effective (*e.g.* pretty, comic, sublime, tragic etc.); and secondly, there are observable certain *conditions of mind* in which we call certain things beautiful, which in other conditions of mind may not appear so, but leave [illegible] interested.

owe the name of the study,—that philosophical pigeon-hole in which it lived till the middle of the 19th century.

Among the multitude of things which we call beautiful or aesthetically effective there is a special class of things, distinguished by the feature that they are made by Man, and (as far as one can tell superficially) are intended to produce this impression—'objects of Art.'

The experience induced by Art-objects is in a narrower sense the subject-matter of Aesthetics. It is its subject-matter *par excellence*, while in other cases the experience may have something accidental or incidental about it.

By Art must be understood not merely the Fine Arts but all human products which aim at producing aesthetic experience, whether they are fine or applied, free or industrial, useless or utilitarian.

Despite the intentional appeal of Art-objects, the conditions in which they produce their effects are variable and diverse, not merely in point of time, but also between different persons and in face of different kinds of objects. Again, the object appears to be—though in an intentional manner—almost one of the conditions of the effect.

What then are these conditions? This is the fundamental problem of Aesthetics.

I remember walking home one night with an eminent scientist, discussing Aesthetics, the existence of which he denied. We met an urchin whistling with obvious enjoyment a tag of 'The Girl from—somewhere or other.' "If you can tell me," said the scientist, "why that boy thinks that tune is beautiful, I will believe that there is such a thing as [illegible]
[illegible]

tion, attention, have been attacked introspectively, retrospectively, experimentally. But a vast amount remains to be done. We possess a great deal of detail, much observation and record; but there is little coordination of all this material. There is a lack, in particular, of a *common stock* of knowledge; and little progress can be hoped for in any study without such a stock of common and accepted truths.

2. At this point it is desirable to refer to two sets of double factors that are involved in all these questions of aesthetic experience.

The first is the observation that the aesthetic experience is clearly not a mere matter of personal, individual concern. The experient does not stand alone in the world, facing its beautiful or ugly objects. His reactions to them are only in part his own. In part they are also the reflexions, the repercussions, of those of others around him, the traditions of his forbears, the conventions of his age. They are in particular and to a large extent the conventions ruling the Art-object, the 'art-conventions' of the moment. In France in the 17th century a tragedy had to be in five acts, and without being in five acts a tragedy had no chance of being considered a good one. To us this seems ridiculous, but our own Art is full of similar conventions. The problem is to distinguish between what is essential and what is merely conventional in Art and in the aesthetic experience which it mediates, and not to be led astray by catchwords and momentary battle-cries.

Until about 1890 Aesthetics was almost exclusively of the individualist type. Since then the collective aspect of the aesthetic experience has received a good deal of attention, notably in France, with the support of the French school of Sociology. The result has been the development of 'Sociological Aesthetics.' The aesthetic experience came to be regarded as a social, not as an individual experience; Art became a social expression, the reflexion of an age, the product of a society. This view appeared to derive strong support from anthropological research which seemed to find convincing proof of it in the Art of primitive and prehistoric peoples.

Whatever be the ultimate truth, so exclusive and exaggerated a view of aesthetic experience is clearly as wrong as the exclusively individualistic theory had been. What we have to deal with is the combination and the interplay of social and individual factors in the aesthetic psychology of the person. What are the relations between them? On which side is the predominance? A most difficult problem, complicated as it is by historical considerations: for the relation has, as a matter of historical fact, not been stable but has changed in course of time.

3. In the second place, when we speak of aesthetic experience, we generally think of the impression made by a picture, a piece of music, a poem. What about the experience that produced these things? What about the Artist? He evidently has a claim to be heard in these matters. After all, he created them. Why? How? What happens when he does create? How does he set about it?—or does it set about him? We are told about 'inspiration,' 'genius.' What are they? Imagination is such a ubiquitous thing, so essential to the most ordinary business of life that we ought to know a great deal about it. We know something about images, imagery, visual and auditory and motor types; but all this is reproductive imagination. About creative, constructive imagination we hardly know anything of practical use.

There is reason to think that for the purpose of aesthetic study the Artist as a type—he is a very clearly defined human and psychological type—may be regarded as enjoying almost pure examples of aesthetic experience, certainly purer, more permanent, more thorough than fall to the lot of the average spectator or listener. From him we ought to secure insights into aesthetic experience far more valuable than from other sources. Unfortunately he is an excessively bad subject. He is not really interested in studying his mental processes, and generally fails to see the point of the scientific attitude to things. He is, as a rule, totally lacking in introspection; he is rarely able—unless he be a literary man or an actor—to express himself verbally with precision and without ambiguity.

The Artist is an interesting object of study, not only in relation to his own aesthetic experience, but in comparison with that of the experient. Indulgent theorists have told us in the past that we are all artists. If words are used with any accuracy, this is nonsense. Nobody is an artist unless he creates; the spectator—*ex definitione*—does not create, and no amount of 're-creating' will make him into an artist. At the same time, there must be certain features common to both types of aesthetic experience, corresponding to the specifically *aesthetic* elements. Any formulation of the experience, therefore, based on the analysis of creative and receptive experience, will have to cover both, showing identities as well as compensating differences.

The French were the first to start a systematic study of the Artist and of his processes, beginning with the elaborate monograph by Toulouse on Zola. Unfortunately this series was never continued, though it was followed by several separate studies devoted to particular artists by Binet in the *Année Psychologique.* A very much larger amount of detailed research is needed, and it can only be obtained by the willing and intelligent co-operation of artists themselves.

To sum up there are two cross-cutting factors to be dealt with: (i) the individual *vs.* the social factor in the experience both of the recipient and of the artist, and (ii) the receptive *vs.* the creative aspects in aesthetic experience.

4. Let us assume that after an exhaustive analysis, research into the individual and social determinants, experiment, observation, reminiscences and introspection, we have reached a formula capable of fitting every kind of experience that is mediated by Art and Nature, and capable of meeting the similarities as well as differences of artistic production and aesthetic reception. Where shall we then stand? We shall have formulated what we mean by aesthetic experience. We shall have a formula—if I may use this word to indicate a relatively stable, pure and probably fundamental type of mind and behaviour—of what we mean by 'aesthetic consciousness.'

But how did this type come into existence? Along what lines has it developed in human history?

This is the aesthetic aspect of the problem of the History of Art and of the Origins of Art, and it is essentially a problem for Psychology.

The problem of the origins of Art is fairly ancient. But it has never had a better chance of being attacked and possibly of being correctly answered than at present, thanks to the great advances of anthropological research in recent years.

It must, however, always be borne in mind that the problem of these origins is not a question only or even predominantly of the origins of those objects which we call Art. It is also and especially a question of the origins of the types and attitudes of mind that created those objects.

The amount of confusion of thought on this topic is truly amazing. Compare, for instance, the mostly valueless stuff written on folk-song alone. The vast collections of mere data, uncritically arranged, compiled without a principle relevant to Aesthetics, incapable of answering hardly a single of its really vital and fundamental questions, are largely useless and constantly suggest opportunities of information missed and so often lost for good and all.

Of course, the problem of the origins of the objects themselves is most interesting, and in so far as it might throw light on the psychological issue, most important. But in themselves the objects supply no answer to this question. Anthropological research led very speedily to the discovery that the greater part of what we call Art—primitive Art—

was *not* Art to the men who made it. And once this discovery had been made concerning primitive Art, a good deal of what hitherto had been considered without question as Art, even in highly advanced stages of culture, has become similarly suspect.

The real question is--how and at what point in the history of any particular culture did the men who made the things produce *Art*? Even the Greeks treated their architects as we treat our masons; *we* call Architecture the 'Mistress-Art' and in the Chinese canon of the Fine Arts we find included therein Archery, Mathematics and Manners.

Here again Aesthetics demands an aesthetically directed psychological research into those internal developments of culture, whereby an originally undifferentiated system of traditional sentiments and conceptions became split into those separate strands of feeling, those distinct systems of ideas, which we have come to term Religion, Magic, Justice, Right, Truth, Craftsmanship, Art. These developments must have taken place —within historical times they are known to have taken place—concomitantly with important external changes in the objects connected with these systems, under the stress of divisions of labour, distinctions of craftsman and public, castes, guilds, priest and layman, the development of media of currency, imports and exports, the effects of wars and travels, interferences of cultures and a number of other factors, which may perhaps all be considered as the outward manifestations of those inward changes which are the true objects of aesthetic research.

Such a research, if successful, should furnish us with the answer to our question, how and why has that mental attitude which we call the 'aesthetic consciousness,' through which and within which we realise aesthetic experience, come into existence, and what has been its history?

The attempt has often been made to fathom the mystery of Beauty by the research into prehistoric and primitive Art, but it has resulted in mere imaginings and wholly unsupported constructions of what the men of Altamira or Neolithic man or the bower-bird thought and felt and meant when producing their 'Art.' It has proved to be an entirely unwarranted substitution of our ideas and views of things for the ideas and views of the men of the stone-age and of certain animals. It has been a vicious circle of the worst description and has meant the search after something which the searchers themselves neither knew nor were able to describe.

5. There is another circular argument of more general application and much subtler in its workings.

Let us again assume that our research, supported by anthropological, material and social-psychological analyses of past and primitive cultures, has furnished us with the whole inner history of that branch of human civilisation which we call Art (which has not always been Art in the eyes of its very creators, but frequently has become Art in those of succeeding generations),—with what one might call 'the morphology of the aesthetic consciousness.'

Is there any check that we could apply to our theories?

We are Europeans and our Art, if we cover the ground best-known to ourselves, most readily accessible and least exposed to misinterpretations, is European, *i.e.* Graeco-Roman in substance and origin. We evolve theories on the basis of this our European experience, and then prove these theories by illustrations and references taken from European Art. Our gratification at the satisfactory results requires severe tempering. Aesthetics has never broken this vicious circle. Can it be broken? Is there any other body of art-experience and material Art, any other culture sufficiently coherent, complex, and conscious, to offer a parallel to ours? There is only one, but an excellent specimen: China. Of even greater age, of the same and even greater coherence, of similar complexity and an equal degree of refinement and critical self-consciousness, Chinese Art and culture, in their at least relative freedom from European adulteration, furnish a unique opportunity for checking our theories deduced from European experience against another experience unconnected in substance, origin and development. It has never been done. We have never had the combination of knowledge requisite for the performance. We are only now beginning to attack problems of Chinese archaeology with modern methods and comprehensiveness, as in B. Laufer's admirable study of jade[1]. We have partial applications of the parallelism as in Prof. Sir W. Ridgeway's recent fascinating paper on ancient Chinese Drama[2]. Much work on archaeology has been done in China recently, but it is not available in translation, and of whole centuries of Chinese history we hardly know anything more than the outward happenings. There is an immense field for work which will have to be done, with results that I venture to think will prove illuminating in the highest degree.

6. What position have we now reached? I assume that we have secured a conception of aesthetic experience as we know it to-day, covering experiences mediated by all the arts, by every kind of aesthetic

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[1] *Jade: a Study in Chinese Archaeology and Religion.* Chicago: Field Museum Publication 154, 1912

[2] "Ancestor worship and the Chinese Drama," *Quart. Rev* April, 1919

object whether man-made or natural, the experience of the artist as well as of the recipient, linked with the social, as well as with the individual, consciousness and tradition.

We have further secured an understanding of its history in human development. We know how that kind of experience arose, first merged in and diluted with other kinds; how it became differentiated at different stages of human history, how at certain moments of particular cultures it appeared in almost pure and unadulterated form—the 'flowering periods' of Art—to be overlaid and obscured at others by other interests, perhaps owing to some slight shifting of the relations of the individual to his community, the intrusion of some other culture, a sudden expansion of intellectual or moral horizons. We have obtained thereby an insight into the inner history of Art and also into the correlation of that type of experience with the other aspects of civilisation, with religion and law, intellectual pursuits, social structure, the conception of right and truth, with the economic and material factors of existence.

There remains the question: what is the relation of this type of experience called 'aesthetic,' to those other types which we designate by the terms 'scientific' and 'ethical'? It is the old problem of the relation of Beauty to Goodness and Truth. This I conceive to be the problem of a *Philosophy* of Art. It is the point at which Aesthetics issues out into the two fields of human interests: human conduct and human ideals. It is the highest point to which psychological inquiry, directed by aesthetic conceptions, into facts of experience can penetrate, where it must join hands at once with practical action and philosophical speculation.

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THE GENERATION AND CONTROL OF EMOTION[1].

By ALFRED CARVER.

(*From the Birmingham Psychoneurosis Clinic.*)

1. *Introduction.*
2. *Emotion aroused in connexion with instinctive processes.*
3. *The function of emotion in relation to interest.*
4. *The rôle of the bodily concomitants of emotion.*
5. *The interaction of the cortex and lower centres in emotion.*
6. *The functional levels at which integration and dissociation may take place.*
7. *Neuroses due to dissociation at various functional levels.*
8. *Anxiety states, conversion hysteria, and Babinski's 'troubles réflexes.'*
9. *Disorders of the ductless glands due to emotional shock.*
10. *Neurotic symptoms resulting from conflict between opposing forces.*
11. *The determination of pathological symptoms by previous trauma.*
12. *The process of sublimation.*
13. *Reasons for here drawing illustrations from military life.*
14. *Conclusions.*

1. THE fact that emotion plays a capital rôle in the genesis of the psychoneuroses has long been recognised; but recently increased attention has been drawn to the subject owing to its importance in connexion with the war neuroses, and the time seems ripe to undertake, in the light of such experience as has recently been gained, a brief survey of the whole subject.

I propose to consider the problem under the following headings:

(*a*) The biological function of emotion.

(*b*) The rôle played by its physical concomitants.

(*c*) The neural site of its production and control.

(*d*) The dissociation of consciousness which may arise in consequence of its excessive generation.

(*e*) The means by which, under conditions of civilisation, emotion may be turned to useful instead of harmful action.

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[1] Read before the Medical Section of the British Psychological Society, 11 June, 1919.

2. Psychologists are now generally agreed that emotion arises in conjunction with instinctive processes. But instinct may be defined in either psychological or biological terms, and one of the great difficulties has always been to keep these clear and separate.

No person has ever yet satisfactorily defined emotion, and any attempt to isolate it from its setting, so to speak, is foredoomed to failure; for emotion is only one part or aspect of a more comprehensive internal adjustment which takes place in higher animals in order to enable them to react as a whole and more completely to dominate any sudden change in their environment. The individual can no longer be regarded as made up of body and mind; he is a biological entity whose activities are manifest at various levels representing successive stages of his evolutionary progress.

For the purpose of clarity, I wish to define the 'interest' of an instinct as the affective tone which accompanies the whole instinctive process when it is carried through in a normally satisfying manner; and to define emotion as the subjective experience which develops when gratification of the instinctive impulse is held in check by higher level control. The reasons which have led me to propose this definition will be made clear in the early part of my argument.

James in his classic work[1] treated instinct and emotion apart, but he added "every object that excites an instinct excites an emotion as well." His well-known view may be summed up in the phrase "I am afraid because I run away."

McDougall[2] has ably argued that emotion is merely the central limb of the whole instinctive process, which like a simple reflex has an afferent and an efferent path. He has thus shown a much truer appreciation of the problem as a whole. Yet his hypothesis merely assigns to emotion a place in a chain of events without suggesting its function, except in so far as its position in the chain indicates a reversion to the common-sense view, viz. "I run away because I am afraid." McDougall's definition further assumes that the 'interest' of an instinct is from the outset always and necessarily an emotion.

That instinctive reactions resemble and are probably the direct descendants of reflex actions has long been recognised. Romanes[3] in particular held that the only point wherein instinct can consistently be separated from reflex action is in regard to its mental constituent.

[1] W. James. *Principles of Psychology*, London, 1905.

[2] W. McDougall, *Introduction to Social Psychology*, London, 1908.

[3] G. J. Romanes, *Phil. Trans. Roy. Soc.*, London, 1877, CLVII.

Discussing the emotional manifestations of organisms at different evolutionary levels, he found that the further one descended in the scale—*i.e.* the lower the mental development of the organism—the more fixed and definite became the instinctive reaction and the less became the manifestation of emotion. Conversely it may be stated that variation of reaction marks the dawn of intelligence. In higher animals the reflexes are under control of mechanisms to whose activity consciousness is adjunct, and the crude affective state is refined until it is recognisable as emotion. Finally in man the enormous development of the neopallium (whose function it is to control the lower mechanically-fatal reflexes and more completely to adapt its possessor to changes in his environment) has led to an infinite variety of possible responses, and correlated with this a maximal disposition for the arousing of emotion. The mere disposition to experience emotion, however, still leaves emotion as an epiphenomenon, without suggesting its function. Yet function is the most important aspect of any biological mechanism and demands therefore our most careful attention.

3. Now a consideration, whether by introspection, or by observations on animals or man, of the circumstances under which the coarser emotions may be aroused leads us to the conclusion that when the conative tendency of an instinct is immediately satisfied the intensity of its emotional component is minimal. The reaction in these circumstances closely resembles a more simple reflex. If, on the other hand, satisfaction is checked, tension arises and the affective element manifests itself and is experienced as emotion. According to this view, in contradistinction to McDougall's, it is only under certain conditions that 'interest' develops into emotion. This explanation harmonizes with the findings of Romanes just mentioned, and with the view of Sherrington[1] that "the primitive emotions seem to involve little other than sense perceptions richly suffused with affective tone." In lowly organisms, indeed, response seems to be massive, on the 'all-or-none' principle, and imperative: it is what we might term a 'protopathic' reaction. On the other hand, with the gradual increase in plasticity of response which develops *pari passu* with intelligence, there is delay in reaction and with this is associated an increase of the emotional component. The longer the issue of the impulse in satisfying reaction is checked by higher level control, the greater becomes the generation of emotion, until in man with his practically unlimited choice of response we reach a stage where delay becomes almost inevitable. Hence it is rare, in man, for response to be unaccom-

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[1] C. S. Sherrington, *The Integrative Action of the Nervous System*, London, 1906, 262.

panied by some generation of emotion. Seeing, then, that it is in connexion with this delay or check to the impulse that emotion arises, we are surely justified in considering that the function of emotion is to reinforce 'interest.' The aroused emotion may be likened to a repercussing force which strives to keep the object in the focus of attention and to insist upon the output of a satisfying reaction although this is now under the control of higher centres.

When satisfaction is persistently thwarted while strong emotional stimuli are continuously applied, symptoms, which in man are termed 'psychoneurotic,' are liable to make their appearance. Of these I shall have more to say later. Under the conditions of modern warfare the soldier exposed to practically incessant danger must remain inactive in narrow trenches or dug-outs, with only very rare opportunities of obtaining a directly satisfying reaction to the stimulus. This being so, one would in accordance with the view just put forward expect neuroses of emotional origin to be far commoner now than in the old days of open warfare and hand-to-hand fighting. That they have proved to be so is an indisputable fact. In 1917 it was calculated that one-third of the unwounded and one-seventh of the total discharges from the British army were permanently unfit on account of functional nervous or mental disorders.

4. Having thus briefly considered the function of emotion, let us see what light recent observations throw upon the rôle of those visceral and somatic changes by which it is characterized. The following three views exist with regard to their significance; the excitation of the corporeal concomitants of emotion

(*a*) is brought about *viâ* the visceral nerve centres, but it is the sensations thus generated through the viscera which give rise to the emotional state in the mind;

(*b*) is excited concurrently with the psychical excitement by the same stimulus:

(*c*) is secondary to the psychical part of emotion.

The first supposition was advanced by James, Lange and Sergi, and the arguments in its favour are set out in the chapter devoted to emotion in James's *Principles of Psychology*. Sherrington[1], however, has shown experimentally that in the dog appropriate spinal and vagal transection, although completely cutting off sensations from the viscera and from all the skin and muscles behind the shoulder, produced no obvious diminution of emotional character, fear, joy and disgust being as readily provocable as ever. This being so, we need not dwell at length upon the first supposition.

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[1] *Op. cit.* 260.

Discrimination between the second and third hypotheses is difficult, if not impossible. The purposive nature of reflex action is one of its most noteworthy features, and it may be argued that the bodily adjustments characteristic of emotion are provocable even when the psychical state remains absent. In this event the same stimulus which elicits what we regard as the expression of the emotion must also concurrently and *per se* excite the psychical state. As Sherrington[1] pointed out, animals reduced to a 'spinal' condition react to stimuli which in the intact animal would produce pain, by movements appropriate for escape from or removal of, the stimulus applied. Head and Riddoch[2] have further shown that the spinal cord in its mass reaction exhibits a number of manifestations which might well fit in with most definitions of emotional responses. Goltz[3] also observed, for many months, a dog whose cerebrum had been ablated. Though this animal appeared in general quite neutral to its surroundings, it was still capable of evincing signs of displeasure or anger. This led Goltz to the conclusion that anger is a more primitive emotion than fear, joy, etc. For, as we might expect, the less highly evolved reactions are localised in the more caudal parts of the central nervous system. Sherrington argues that the retention of the expression of anger by Goltz's dog indicates that by retrogression the complex movements of expression may, in certain coarser emotions, have passed into a simple reflex act, and that the stimulus originally arousing strongly affective ideas may come through the canalising force of habit to cause a discharge of the act before it can be apprehended as an idea[4]. The pseudo-affective reactions indicative of resentment are, however, short-lived after ablation of the cortex, the simulacre of mere flashes of mimetic passion.

May it not be that after removal of the higher centres the original reflex reaction, upon which a controlling superstructure was subsequently built up, continues unchecked, though with the removal of the controlling influences the emotional component of the reaction ceases? In this case there is no cerebral reverberation and the response is massive, fixed and short-lived.

A somewhat analogous and more easily studied mechanism is met with in the act of micturition. Here we can trace the acquisition, by the child, of voluntary control over a simple reflex action and, in cases of subsequent interference by injury with the paths through which control came to be exercised, the dissolution of the more highly organized products of mental and neural activity leaving the more lowly at liberty again to express themselves freely. According to the completeness of the dissolution the interference manifests itself on the effector side, on the afferent side, or on both[1].

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[1] *Op. cit.* 266.

[2] H. Head and G. Riddoch, *Brain*, 1917, XL. 188.

[3] F. Goltz, *Pflugers Archiv*, 1892, LI.

[4] Sherrington, *Op. cit.* 267

In view of these general conclusions we may consider that although visceral and somatic sensations undoubtedly contribute to reinforce emotion—probably by a process of repercussion which will be considered later—they are not its actual excitants, and that as compared with the cerebral reverberation to which the psychical experience is adjunct, they count for very little even in the coarser emotions of the dog.

It may then be asked what is the rôle played by the bodily concomitants of emotion? Numerous suggestions have been offered to account for them, but all the more recent experimental observations demonstrate that these bodily changes are of such a nature as to put the organism physically in the most advantageous condition to respond effectively to affective stimuli.

Cannon[2] and his co-workers in particular have shown that every one of these visceral changes is directly serviceable in making the organism more effective in the violent display of energy which fear, rage, or pain may involve. The changes are mostly the immediate result of sympathetic activity with which the action of the endocrine glands is associated. But Orr and Rows[3] have shown that "the sympathetic system is intimately linked up with the central nervous system and has no true autonomy." Thus the fact that the visceral adjustments are brought about *viâ* the sympathetic system in no way conflicts with the theory of the cerebral origin of emotion. Cannon[2] has shown by experiments on cats that psychic excitement can through sympathetic paths bring about a hypersecretion of adrenin. The action of adrenin seems identical with that of the sympathetic nervous system itself, and causes adjustments in the economy of the organism which favour an increased energy output. Either directly or indirectly adrenin raises the blood pressure, alters the distribution of the blood in the body, stimulates the heart, dilates the bronchioles, restores to fatigued muscle its normal irritability and liberates sugar into the circulation. These are precisely the phenomena which we meet with as manifestations of the primitive emotions, and their utility to the animal in enabling it to respond effectively to the stimulus is self-evident. They are precurrent reactions, but cannot be regarded as originating the psychic component of the emotional complex.

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[1] E. G. Fearnsides, *Brain*, 1917, xl. 184.

[2] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, 1915.

[3] "The Interdependence of the Sympathetic and Central Nervous System," *Brain*, 1918, xli 15.

5. In approaching the problem of affective states by experimentation on animals we are, unfortunately, cut off from introspective help, but disease in man sometimes enables us to study the problem without labouring under this impediment. Head and Holmes[1] have taken advantage of this fact by observing the alterations in affective state in patients suffering from lesions of the cerebral cortex and other parts of the brain. The conclusions arrived at by these observers are that the optic thalamus contains the terminal centre for all sensory stimuli, and that from here the latter are regrouped and distributed in such a way as to act both upon the essential thalamic centre and the sensory cortex; the thalamic centre itself being especially concerned with the affective aspect of sensation, while the cortex exercises discriminative and inhibitory functions.

Not only does emotion, once aroused, serve to bring into play associative memories and stored-up stimuli, but at the same time there seems to be a diminution of the higher control normally exercised through the cortico-thalamic tract; the mechanism here being comparable to, if not identical with, that of 'reciprocal inhibition with simultaneous double sign.' A similar phenomenon is also observable in the inhibitory influence which the cranial and sacral divisions of the automatic system exercise over the sympathetic proper. Again it is well recognised that, in the motor system, there is a delicate adjustment between the reactivity of the upper and lower neurones, and that impaired function of the upper neurone allows more final common paths to be forced in the lower. The relatively few observations which have been carried out indicate that a similar relationship pertains in the sensory system.

6. Another very important aspect of the work of Head and his collaborators is their demonstration of the way in which integration can occur at all afferent functional levels of the nervous system. They show that the struggle of the higher centres to dominate instinctive tendencies is begun at the lower afferent levels, but becomes more clearly defined the nearer sensory impulses approach the field of consciousness. This may be looked upon as a corollary to the statement by Romanes referred to previously.

The question as to the level at which the converse process, viz. dissociation and loss of control may occur in the neuroses, is one of considerable interest, especially in view of its bearing upon the class of phenomena which Babinski[1] has labelled 'reflex.' Bechterew and Pighini[2] hold the cortex cerebri itself to be made up of many sensori-motor levels, each in functional and anatomical connexion with neighbouring centres. And as Sherrington[3] says, "each synapse is an apparatus for coordination, it introduces a common path." There would seem no inherent reason against the hypothesis that functional dissociation may occur at any level, while there is much evidence in favour of such a view.

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[1] "Sensory Disturbances from Cerebral Lesions," *Brain*, 1911–12, xxxiv.

In this country as I have urged elsewhere[4], the attention of observers has been too exclusively directed to the psychical aspect of the war neuroses, and the underlying physical basis of disturbances has been too generally neglected. Head[5] truly remarks that "the day of the *a priori* psychologist is over....A man can no longer sit in his study and spin out of himself the laws of psychology by a process of self examination. For we have been able to show that, at a level deeper than any he can reach by introspection, are prepared those states which condition the nature and characteristics of ultimate sensation....At each anatomical situation in the central nervous system dissociation assumes forms appropriate to the functional combinations of afferent impulses at that point. ..Finally a lesion of the cortex causes changes which can be expressed in psychical terms only."

7. There is a general consensus of opinion that the neuroses, both those of peace and war, arise as a result of functional dissociation due to loss of higher level control, and that the most common cause of this loss of control is emotional in origin. But considerable controversy exists as to the manner in which this operates in the production of the symptoms. Let us then consider this point in the light of the foregoing conclusions. Emotion having as its function the reinforcement of the 'interest' associated with an instinctive process, it is, as I have endeavoured to show, only natural that, when the issue of the impulse in satisfying response is continuously withheld while the stimulus remains active either in actuality, in memory, or in imagination, the result can only be injurious to the organism. In practice we find that the injury

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[1] Babinski et Froment, *Hystérie, pithiatisme et troubles réflexes*, Paris, 1916.

[2] Quoted by Orr and Rows, *loc. cit.* 19.

[3] *Op. cit.* 351.

[4] *Proc. Roy. Soc. Med* 1919, XII.

[5] "Sensation and the Cerebral Cortex." *Brain*, 1918, XLI. 177–179.

manifests itself by disturbances of function in one or more of the following directions—psychic, somatic or visceral.

As already mentioned, the unprecedented conditions of modern warfare are just such as are calculated to arouse intense emotion by withholding satisfaction from the instinctive processes so violently stimulated. The emotional tension, however, continues to seek an outlet; and this being denied to it in a normally satisfying direction, the organism endeavours to escape, or to free itself from its unbearable condition by elimination of the disagreeable stimulus. This it attempts to do by withdrawing attention from the stimulus, *i.e.* from the object or idea, a process technically known as 'repression.' Unfortunately, perhaps, this seems seldom to be wholly successful; for when the focus of attention sweeps on to some associated object or idea, the energy properly belonging to the original process, instead of disappearing, has a tendency to attach itself to the latter. Myers[1] gives a good example of this in a soldier in whom the original emotion had been aroused in connexion with a rifle, but by displacement of attention to a series of associated objects it spread first to the bayonet and hence to similar sharp weapons such as knives, until finally the patient was afraid even to use his razor.

It often happens that this process—known as displacement of the affect, but really due to displacement of attention—is arrested at some particular stage, thus giving rise to a simple phobia, of which the case just quoted is an excellent example; but it may continue to spread until a state of general anxiety is reached. In such cases it seems irrelevant what comes to be feared so long as something other than the original excitant of the emotion is feared. The less conscious the source of the original emotion, the greater is the tendency to dissociation. In the war neuroses the original source of emotion is rarely completely unconscious, hence the well-recognised ease with which the soldier can be 'cured' as compared with civilian cases.

When, then, through the mechanism just described, the pent-up emotion finds a vent at the psychic level, it produces either a simple phobia (or some analogous symptom) or a general anxiety state. If, however, the outlet is somatic, the symptom manifests itself as a bodily process, termed by Freud[2] a 'conversion hysteria.' MacCurdy[3] defines this as "a neurosis in which there is an alteration or dissociation of consciousness regarding some physical function." The symptomatology of this group is infinitely varied, but practically all the symptoms are such as will secure the patient relief from the conditions which brought about the emotional stress on which they indirectly depend.

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[1] *Present-Day Applications of Psychology*, London, 1918.

[2] Breuer and Freud, *Studien uber Hysterie*, Wien, 1895.

[3] *War Neuroses*, Cambridge, 1918, 87.

In the group of cases now under consideration we are confronted with dissolution, the reverse process to evolution. The most recently acquired and most complex integrations are the first to be abolished. Loss of control beginning at the highest levels spreads down by gradations, each of which represents a successive step in evolution. The variety of conversion symptoms seems unlimited, but the essential mechanism of them all, whether they take the form of aphonia, stammering, heart-neurosis, or the more gross contractures and paralyses of the limbs, is explicable upon the same fundamental basis. There has been an overflow of energy which has abolished higher control, has disturbed coordination and the balance of reciprocal innervation, and has forced open unusual and undesirable final common paths.

8. Myers[1] drew early attention to the fact that, while conversion hysteria is most common in private soldiers and N.C.O.'s, the anxiety states are met with chiefly in officers.

MacCurdy[2] and Rivers[3] have laid later emphasis upon this and have attributed it partly to the general difference in early education, partly to the difference in the character and effects of military training and duties. Anxiety states may be regarded as arising at a higher mental level than does conversion hysteria. The higher ideals and greater responsibility of an officer cause him to repress all manifestations of fear and to endeavour so to act before his men as to set them an example. The fear which originally arose in connexion with the instinct of self-preservation thus becomes displaced and attached to the idea of his responsibility towards others; its original cause becoming *bewusstseinsunfahig*. Finally it may come to manifest itself as a general anxiety state.

In the private soldier the conflict between self-preservation and those forces which have been summed up under the term 'herd instinct[4]' is, as a rule, fought out at a lower mental level and the tendency is for somatic rather than psychic symptoms to develop.

The functional level at which dissociation may take place extends as we have seen even to that at which visceral and sympathetic symptoms make their appearance, though Babinski[5] has pleaded that in such cases—a full consideration of which would lead us too far afield—the trouble is of peripheral reflex origin. There is little to support Babinski's contention, while apart from its intrinsic improbability Roussy and others have adduced evidence which strongly opposes it. I prefer then to regard the visceral and sympathetic symptoms met with in certain of the psychoneuroses as indicating that dissociation has, in these instances, taken place at a lower functional level.

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[1] *Lancet*, 9 Sept. 1916.

[2] *Op. cit.* Chapter x.

[3] "War Neurosis and Military Training," *Ment. Hyg.* 1918. II.

[4] W. Trotter, *Instinct of the Herd in Peace and War*, London, 1915.

[5] *Op. cit.*

9. We are not unfamiliar in civil practice with the onset of certain morbid conditions as a result of severe emotional shock, and Crile[1] has drawn attention to them under the head of 'kinetic diseases.' Mott[2] notes that about 10 % of the cases of severe war neuroses under his care showed symptoms of Graves's Disease. It would seem that when excessive disturbance has carried dissociation to this level the resultant disorder is usually much more difficult to bring under control again. I have seen, in the Special Military Neurological Hospitals, cases apparently identical with milder forms of Graves's Disease persisting for months. Yet psycho-therapeutic treatment, though slow, seemed beneficial. We must, however, reckon with the possibility that a severe nervous or emotional shock resulting in dissociation which manifests itself at this level may be capable of causing a permanent disorder in tissues normally under sympathetic influence.

That a delicate balance exists between the functions of the various endocrine glands is well known though imperfectly understood[3]. Crile asserts that there is a chemical interaction between the adrenals, the thyroid, and the brain. In this way he brings together the theories of nervous and humoral repercussion.

When suitable response to the stimulus can be made and vent thus afforded to the emotional and physical tension aroused, this two-fold repercussion would, on the lines previously discussed, be of great advantage to the organism. But the result of excessive outpouring of chemical excitants where no opportunity for their use is given might be expected to damage not only the glands themselves, but other tissues also. It is along these lines that we may profitably look for an explanation of some of the more obscure disorders to which violent and prolonged emotion gives rise.

10. With shock of commotional origin I am not here concerned[4],

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[1] *The Kinetic Drive*, 1916.

[2] "Discussion on War Neuroses," *Brit. Med. Journ.* 2 April, 1919, 441.

[3] W. Falta, *The Ductless Glandular Diseases*, Trans. by Meyers, 2nd edit., Philadelphia, 1916.

[4] I have discussed this question in an article entitled "Some Biological Effects due to High Explosives," *Proc. Roy. Soc. Med.* 1919, XII.

neither do I propose to deal with predisposing causes such as 'mental make-up' or fatigue, important though the latter be.

Turning now to the last aspect of our subject, and assuming with Lloyd Morgan[1] that "the primary object and purpose of consciousness is control," and with Head and Holmes[2] that "the aim of human evolution is the domination of feeling and instinct by discriminative mental activities," it remains to find a means by which this control may be achieved. Fortunately we find the means of control to be implicit in the very mechanisms which, if the foregoing conclusions be substantially correct, are operative in the production of emotional disorders.

Brief though the survey of so wide a subject has necessarily been, we have seen that the dynamic trend of the individual is evolved through and continues to manifest itself at successive functional levels: first, the level to which belong, for example, the diffuse chemically-conditioned reflexes of the endocrine glands; secondly, the sensori-motor or spinal (including lower brain) level, through which we can trace the integration of simple reflex actions gradually culminating in complicated instinctive processes; thirdly, the psychic or cortical level, which is built up on the lower and exercises a controlling and inhibiting influence upon them. At the last level consciousness is present which, in the words of Bergson[3], "signifies hesitation or choice." And it is in connexion with the check upon primitive responses, imposed by consciously directed action, that emotion properly so-called arises. We have also seen that, when the conative tendency is thwarted or repressed, the emotional component of the instinctive process tends to find expression by attaching itself to associated ideas in order that a common outlet may be found. That is to say, the symptoms are the result of a compromise between two conflicting forces,—the instinctive impulse which is striving for gratification along primitive but now unacceptable paths, and the higher level control which blocks these paths.

11. Now if the selection of the associated idea be left to chance it is more than likely that the compromise outlet will be an undesirable one. Often an apparently new association will be found to be dependent upon some old disability. Thus in a soldier who in earlier life suffered from wrist-drop, any wound of the arm associated with conditions capable of producing emotional shock will be liable to re-establish the old disability as a functional wrist-drop. When careful histories are taken innumerable instances of this sort are discovered. Again the mere fact

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[1] *Introduction to Comparative Psychology*, 1894.

[2] *Op cit.*

[3] H. Bergson, *Creative Evolution* (Eng. trans.), 152

of having seen some comrade suffering from a particular morbid process may induce the patient to imagine that his condition is of the same order. I remember an officer who had seen a friend dying from cerebrospinal meningitis develop later, when worn out by physical and mental fatigue, the signs of that disease including marked retraction of the head, which however all disappeared when he was reassured that he was only run down. It is common to speak of symptoms arising in this way as due to suggestion, but I cannot see what use is served by the introduction of this already vague term. In any case we are only dealing here with an exaggeration of a normal mechanism[1].

12. The apparently chance selection of the associated idea, which serves to determine the final common path in cases of emotional overflow, is, however, amenable to control in the following way. If a suitable and generally acceptable psychical equivalent is found, and by education is deliberately substituted for the primitive instinctive tendency, the energy of the instinct and its reinforcing emotion can be diverted into this channel along which its discharge is then beneficial. Such a process is termed 'sublimation.'

Freud has shown[2] that most of our social activities are sublimated forms of instinctive tendencies the direct expression of which would be unacceptable to our present standard of civilisation. In the past this has come about unconsciously or unwittingly, but now that we are in a position to analyse, and in part to understand, the process involved, there is no reason why education, in the broadest sense of the word, should not consciously be directed to the more complete attainment of this goal. The desired result is, as has been shown, not to be achieved by efforts to suppress the energy attached to instinctive processes, but by a utilisation of this very energy in directions which, under present social conditions, are either harmless or of positive value. Unfortunately it does not seem possible to divert this energy into any given channel at will. Experience shows that the new channel must be psychically equivalent to the old; that is to say the original and the new outlets must bear some relation, either conscious or unconscious, to one another in order that the discharge along the new path may afford, to the individual, a satisfying reaction.

The value of sport has long been recognised in the curriculum of boys' schools and in the training of adults. Looked at from the point of view just advanced, its mode of operation is obvious. In this instance the energy of the primitive combative or aggressive instincts is afforded a similar outlet. A further step is taken when the combative instinct is given a purely mental outlet, as in debate. One may say, as a general rule, that the more the new path resembles the old the more easily is sublimation effected[1] and the less liable is it to break down. Thus an important football match is less likely to be 'foul' than an important debate to be acrimonious and vituperative.

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[1] A Binet, *Les altérations de la personnalité*, Paris, 89.

[2] *Drei Abhandlungen zur Sexualtheorie*, Wien, 1905.

The process of sublimation only occurs at a great expense of energy, and if for any reason the resistance of the new path becomes too high the energy reverts to a more primitive channel, a regression which is a constant feature in the neuroses. In our daily lives we derive the energy for our individual struggle for success from instinctive sources, and the degree to which each of us observes fair play and succeeds in maintaining the struggle depends partly upon his upbringing or training and partly upon whether the more refined means now at his disposal are really satisfactory psychical equivalents for the cruder and more primitive methods of expression.

An amusing outcome of sublimation is afforded by the pacifist movement, for the pacifist in waging war against war is drawing the energy for his campaign from the self-same source—the combative instinct—which originally led to an exactly opposite result. As the conditions of modern warfare fail to afford a satisfactory outlet for the primitive instinct, its sublimation is facilitated, and signs are not wanting that the pacifist eventually will emerge victorious. If his victory is to result in a permanent peace, some means other than war must be found along which international rivalry can discharge itself. Many such suggest themselves[2], but it remains to be seen whether they will prove satisfactory psychical equivalents for the original reaction.

13. Throughout this paper the illustrations and deductions have been drawn from military life because here we are confronted with the basal facts in a much cruder form than they are usually to be met with in civil life, and their analysis becomes so much the easier.

Unless the principles which arise out of a consideration of the problem of the war neuroses hold good over wider fields of knowledge they can be but partial expressions of the truth. But in my experience the principles deduced above do hold good over the whole field of so-called functional or psychogenic disorders. In spite of many complications we can disclose the conflict which has resulted in the repression and dissociation of one set of antagonists—that set which appears to the ego as unbearable—and has displaced the energy properly belonging to the latter along channels of less resistance onto some associated idea or action.

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[1] At the same time the two paths must not be practically identical, for in this event no true sublimation is possible.

[2] Cf. W. James's *Moral Equivalents of War*.

The further step of tracing back, by psychological analysis, these associations to their roots is too big a task to be attempted here, but from the therapeutic point of view its importance must not be overlooked.

14. The following conclusions have been reached:

(1) Emotion is only one aspect of the internal adjustment which an organism makes in order more completely to adapt itself to sudden changes in its environment. The function of emotion is to reinforce the 'interest' of an instinct as higher control over mechanically-fatal massive responses develops and checks the latter.

(2) The visceral and somatic concomitants of emotion are not responsible for originating the affective state, but are anticipatory physical adjustments which enable the organism to put forth all its energy effectively to satisfy the instinctive process stimulated.

(3) The optic thalamus is the centre of consciousness of the emotional state. Its activity is normally held in control by discriminative activities arising in the cerebral cortex.

(4) Dissociation is merely the obverse side of integration, and may take place at any level at which, phylogenetically or ontogenetically, integration has been brought about. The latest acquired and most complicated integrations are the most easily dissociated.

(5) The energy of an instinctive process can find outlet along psychically equivalent paths, but attempts entirely to thwart satisfaction lead to apparently fortuitous 'displacement of the affect' and its attachment to associated ideas, and they are only too likely to result in manifestations comprehensively termed psychoneurotic. Ultimately then our problem resolves itself into the finding of useful psychical equivalents and the inculcation of these as desiderata. The earlier in the life of the individual education along such lines is begun, the easier will be the process and the stronger and the more permanent the result.

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THE RELATION BETWEEN THE WORD AND THE UNCONSCIOUS.

BY JOSHUA C. GREGORY.

1. *Intuition and perception.*
2. *Intuition and the unconscious.*
3. *The unconscious not a duplicate of the conscious.*
4. *The inadequacy of the 'stage and actors' metaphor.*
5. *The relation of the conscious and unconscious in image and meaning.*
6. *The relation in memory.*
7. *Words in relation to meaning: meaning as unconscious reaction.*
8. *Fallacy in the notion of 'signs' and 'marks.'*
9. *The word as a directive stimulus to the unconscious.*
10. *Suggestive signs not meaningless.*
11. *The distribution of attention to words and meanings: the relation of signs to the unconscious.*

1. "There is...a sagacity which...does not wait for the slow process of deduction, but goes at once, by what appears a kind of intuition, to the conclusion." Hazlitt adds this sentence from Sir Joshua Reynolds to his own statement that "In art, in taste, in life, in speech you decide from feeling, and not from reason; that is, from the impression of a number of things on the mind, which impression is true and well-founded, though you may not be able to analyse or account for it in the several particulars." "Common Sense," he adds, "is the just result of the sum-total of such unconscious impressions in the ordinary occurrences of life, as they are treasured up in the memory, and called out by the occasion[1]." All mental process tends to assume the form of intuition by condensing into one moment of apparently unsupported insight the effects of experiences dispersed through the mind's past life or of deductions once deliberately and consciously drawn from such experiences. External perception shows this very clearly. The mind *sees* that a stone is hard, though hardness, as such, can affect only the sense of touch. This visual intuition or insight is really supported by memories of contacts with

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[1] *Table-Talk*, Essay 4: "On Genius and Common Sense."

objects that looked like the stone and by memories of sensations received from these objects when they were touched or handled. Visual perception becomes intuitional by dropping all conscious reference to experiences of touching hard objects, and instantaneously inferring their hardness from their appearance to the eye.

This acquired intuitional insight "does not wait for the slow process of deduction," with its labour of recalling past occasions when objects that looked like the stone were discovered to be hard by touching them, and with its effort of inference from these memories of the past to the present situation. It accepts the risk of illusion at the price of increased rapidity. Cardboard, shaped and painted like rock, can deceive the eye. This danger of error pervades all varieties of intuition since it necessarily arises whenever an index, like the index of hardness provided by the look of the stone, is assumed to be infallibly connected with the qualities it is employed to indicate. This assumption of invariable connexion becomes particularly precarious when the mind steps outside the ordinary run of its experience: a Highlander, accustomed only to the rocks of his native mountains, might be misled if suddenly placed among the mimic rocks of a city theatre. Protective mimicry in the animal world probably exploits the possibility of error accepted by perception in following its intuitional habit. Birds avoid many insects that sting or are unpalatable, by accepting their gaudy garb as an index of disagreeableness; insects that are really toothsome morsels are left alone if they adopt the same gaudy garb. The intuitional method is the very soul of mental perception, which employs impressions from the external world as hints, transforming them so rapidly and spontaneously into deductions that the mind has to reflect carefully to become aware that they are deductions. Hardness is inferred, not seen; the whistle is heard but the train is deduced; "I came home to my fortification," Crusoe wrote of himself after he had seen the footprint in the sand, "not feeling, as we say, the ground I went on, but terrified to the last degree, looking behind me at every two or three steps, mistaking every bush and tree, and fancying every stump at a distance to be a man." Crusoe had perceived a footprint and *seen* that savages had landed on his island.

2. It seems quite evident that a conscious perception or a conscious idea of any kind is often, if not always, the conclusion of an unconscious mental process. It is a terminus, the momentary ending of a process sustained by the accumulated and organized impress of past experiences on the mind. The mind's tendency to work under the form of intuition depends on its innate habit of using past experiences in an unconscious

way to produce a conscious result, it is, in fact, the natural result of this process. This unconscious working is observed to pervade all mental process the more widely and deeply, the more attentively that mental process is studied. *Seeing* the hardness of stone can be regarded as the conscious result of a rapid, unconscious summary of past occasions when sensations of hardness were consciously associated with certain visual appearances. To suppose that this *conscious* association, equivalent to a deliberate inference that a stony look means a hard object, ever existed is probably to attribute to the conscious what belonged to the unconscious: consciousness surely accepted the stony appearance as an index of hardness in an automatic or unconscious way. Intuitions or ideas consciously *received* into the mind seem often to be "the just result of the sum-total of such unconscious impressions in the ordinary occurrences of life." Hazlitt, in the same essay from which this quotation is taken, describes how a man who had been arraigned in 1794 on a charge of high treason, retired to Wales to write an epic poem. One morning, as he sat at breakfast, a man passed his window. He was instantly haunted by visions of prison, trial and the gallows. He did not recognise the man, but he had actually seen the face of Taylor the Spy. Unconsciously received impressions thrust into his mind the fear that he had been marked down.

3. Because the unconscious workings that terminate in conscious processes cannot be seen or touched or known by direct apprehension, they can be, and often have been, denied. They are deduced because they provide a meeting-place for the convergence of lines of inference that would otherwise hang out, so to speak, in empty space. Bleuler[1], summarising some of these lines of inference, remarks that walking is directed by sensations, by sensations from contacts between feet and ground, by sensations constantly proceeding from moving muscles, tendons, joints etc., that do not press into consciousness, though a trip or a stumble may suddenly remind the walker that he is depending on them. If the walker be immersed in a problem he may thread his way, even change his route, without being consciously aware of the impressions of sight, touch or sound that are guiding him, which might have been consciously received and reacted to if his attention had been less distracted. Impressions that would have been conscious normally act in an unconsciously mental way. He may be heedless, adds Bleuler, during his walk, of some incident that afterwards appears suddenly in his conscious memory. A girl under hypnosis repeats an Arabic dedication which

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[1] *Studies in Word-Association*, VI.: "Consciousness and Association."

she neither understood nor remembered on waking. Past scenes, past thoughts, past dreams can remain outside consciousness and, after many years, re-enter it: Merz[1] remarks that memory *compels* an account with the unconscious. Recollection and reminiscence are constant reminders that past experience is constantly in unconscious, or subconscious, operation; and (as will shortly appear) memory both points a steady finger in the direction of the unconscious and suggests false descriptions of its nature.

This combination of correct deduction with false description may have prompted those flat denials that have been, and still are, given to the suggestion that mental process may be unconscious. Bleuler, referring to those who consider it nonsense to speak of *unconscious* mental process, compares the psychologist who confines the mental to the conscious to the biologist who would only study the terrestrial life of amphibia. A belief, of course, is not proved absurd by transforming it into a metaphor. He draws a reasonable inference from the apparent convergence of lines of evidence on unconscious mental process when he affirms that there are some functional formations and mechanisms outside as inside consciousness and as capable of determining the psyche. In his *Psychological Principles*, Dr James Ward implicates the conceptions of the unconscious and the subconscious in the concept of the 'field of consciousness.' At the focus of the field, to which the eye of attention is directed, consciousness is at a maximum: Archimedes is absorbed in his figures on the sand. The 'din of the siege' falls vaguely in the outskirts of consciousness, consciousness falls steadily towards a minimum from the centre of the field to the periphery. The law of continuity naturally suggests that beyond this periphery lies a "volume of subconscious experience supporting the field of consciousness and vitally continuous" with it. Dr Ward distinguishes between the unconscious and the subconscious: "We cannot fix the limit at which the subconscious becomes the unconscious." Bleuler, conveniently and reasonably, even if Dr Ward's triple distinction into conscious, subconscious and unconscious be finally allowed, directly opposes the conscious and the unconscious; but he proceeds too hastily when he adds that there are in this sense (referring to unconscious functional formations and mechanisms) sensations *only* distinguishable from similarly named conscious phenomena by the absence of conscious quality. In insinuating this error, memory is like a pretended deserter who truthfully tells his captors that their enemy has lines in front of them and falsely describes their dispositions.

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[1] *A History of European Thought in the Nineteenth Century*, 1912, III. 289.

Memory says truthfully that there is an unconscious mind; it insinuates a very false description of it by suggesting that there is an unconscious duplicate of conscious life.

A chemist recognises the presence of sulphuric acid by obtaining a fine, white precipitate when he adds to the suspected liquid, acidified with hydrochloric acid, a solution of barium chloride. He learns to draw this inference by observing that he obtains his precipitate when sulphuric acid is known to be present and does not obtain it when sulphuric acid is known to be absent. His process of inference rapidly becomes an intuitive connexion between the appearance of the precipitate and the presence of sulphuric acid: he ceases to think consciously of former occasions when he applied the test. His establishment of the precipitate as an index of sulphuric acid contains, like all intuitive processes, a risk of error: sulphuric acid may not be, and is not, the only substance that responds to his test. Memory, recollection, enables him to retain a critical control over his intuitive process. His past experience, acting unconsciously, determines his conscious inference that sulphuric acid is present when he obtains his precipitate; but he can, if he so desire, recall past experiments—recollecting the circumstances under which they were successful or that certain substances, which might now be interfering, were absent in them. Since he can recall past experiences into consciousness it is quite natural to assume that when they unconsciously determine his conscious procedure they exist just as they were first experienced, or just as they were remembered, only unrecalled. All recollection tends to deceive in this way. A man is powerfully influenced by certain thoughts that come into his mind. For weeks, for months, for years these thoughts control his actions or direct all his thinking without coming into consciousness as actual memories. Then he suddenly recollects these thoughts and realises how they have determined his life. He naturally supposes that they have, *as such*, lain in his unconscious mind and *as such* unconsciously influenced him. This illusion is probably assisted by an analogy unconsciously adopted from perception of the external world. We think of a mountain that we have seen and left as existing just as it was and is till we happen to see it again. In the same way we think of the memory of the mountain as permanently lodged in our mind, always the same and merely coming under attention when we think of it at some odd moment. The external world thus becomes the pattern of the world of ideas. Just as material objects remain after we have perceived them for us to perceive again, so perceptions that we have once experienced or ideas that have once entered our consciousness remain, or may remain, in our minds till we again notice them or until they force themselves again upon our conscious attention. This analogy more explicitly applied determines such statements as Bleuler's that there are sensations *only* differing from their conscious equivalents by being unconscious.

4. Consciousness is determined by the unconscious when a thought, after being consciously received, continues to affect the processes of the mind without being explicitly thought or remembered. Its unconsciously exercised influence results from its first conscious reception, so that the unconscious, in its turn, is determined by the conscious. This mutual relation of determination between conscious and unconscious must be clearly recognised in all attempts to conceive the connexion between them: it serves also as a first effort to conceive the character of the unconscious. When an incident is seen and subsequently remembered, nothing more is implied than that conscious perception so affects unconscious process that it may result, at some future date, in a conscious reference to the original perception. Recollections of past incidents usually do only appeal to us as references to the original perceptions; when they succeed in posing as the perceptions themselves they are described as illusions or hallucinations. Thus even recollection, which does insinuate that ideas pass to and fro between unconscious and conscious, as external objects pass into and out of attention, is unable entirely to conceal the difference in nature between conscious and unconscious. Language, constantly wedded to meanings it has acquired through its application to external objects, tends constantly to back the insinuations of recollection against its failure to justify them. It is very natural to speak, with Hazlitt, of impressions being "treasured up in the memory" and of their being "called out by the occasion." Such phrases are inevitable, they may even be said to give a welcome concreteness to expression, but they do, through their habitual associations, convey the suggestion that consciousness is like a stage and ideas like actors that enter and leave it: actors in the play resembling ideas in consciousness and the same actors out of the play resembling the same ideas in the unconscious.

This conception, prompted by recollection and suggested by language, of the unconscious mind as a store, is a convenient working concept; but is a misleading notion if persisted in as a true or final account. It will obviously be difficult to frame an adequate concept for the unconscious. It is difficult to secure an adequate conception of the externa world that can be apprehended by many minds in common. It is more difficult still to conceive adequately of the individual mind whose conscious ideas can only be directly apprehended by itself. The conscious thoughts of an individual can, however, be laid directly open to inspection by expression in speech or writing or in significant movement. Conscious thought, indirectly, partially and fitfully becoming part of a world common to many minds, appears to be intermediate between the material world that lies freely open to all and the unconscious minds that lie outside even their own consciousnesses. The use of the 'stage and actors' metaphor to represent the relation between conscious and unconscious gives an initial mental grip upon the latter. Preliminary working concepts fulfil one function by giving this initial mental grip; they have a further function in rousing a sense of their own insufficiency. Simple persistence in regarding conscious ideas as constant permanent entities that pass in and out of consciousness, as birds fly in and out of a nest, soon directs thought to think of the unconscious, more vaguely perhaps, certainly less picturesquely, though with less error, as mental process determining the conscious, and, in turn, determined by it.

5. Since the conscious and the unconscious are everywhere in mutual determination, their relation can be studied at any psychical point. It can be studied in the structure and movement of the concept or idea—more particularly in the relation between image and meaning. This relation between image and meaning reiterates the intimation given by memory that unconsciousness underlies consciousness and repeats the error of recollection in insinuating that unconscious ideas are simply conscious ideas unattended to or deprived of conscious quality. More correctly, one aspect of the intimations of memory is presented in the meaning-image relation. When the idea of a 'horse' is aroused in the mind, usually as part of a wider context such as 'the horse will gradually become extinct as motor travel and traction develop,' it has a very small proportionate representation in consciousness. The word 'horse,' either as a visual image corresponding to the printed word or as an auditory image corresponding to the sound of the word spoken, may be the sole conscious aspect of the idea that is certainly wider than the mere word visualised or heard. Faint mental pictures of a horse and various 'kinaesthetic' images can be disregarded since, if they should be present, they only make the conscious imagery a little more expanded and complex: the visual image of the word h-o-r-s-e can be used to denote the whole imagery, actual or possible, of the idea. The printed word h-o-r-s-e has no significance for one ignorant of its meaning, neither has the mental image, as such, any significance either. The whole idea con-

sists of the visual image of the word, presented in consciousness, and the meaning it subserves. This meaning, stating an antithesis that is not too sharp for adequate analysis though it may not be quite so sharp in reality, falls completely within the unconscious. An attempt to explain the word 'horse' or to discover to ourselves its complete meaning shows this clearly. When we read or hear 'the horse etc....' we do not consciously think of a certain shape or size, or of eating corn or of drawing guns or of prehistoric ancestry or of all the many items that rise into consciousness when we explain or analyse out what 'horse' means. All this meaning, we thus realise, has operated unconsciously on the process of our thought through the visual image 'horse.' This analytic process, during its revelation of the unconscious character of 'meaning,' induces us to believe that our conscious recollections are lifted as such out of the unconscious region of meaning into the region of consciousness. It appears to us as though the visual image 'horse' called upon the recollection of 'drawing guns' and upon all other recollections associated with it in the meaning of the image to stand to attention. If the sentence run 'the horse is used for drawing guns,' one of these recollections, roused by the word horse to readiness to enter consciousness, does so enter; if the sentence run 'the horse eats oats,' another recollection, also roused to readiness, takes its cue and takes its turn in consciousness, as an actor treads the stage when his call comes.

6. Memory, in the widest sense of the term, is the organized disposition, or dispositions, to react, impressed upon the organism by its experience. This organization of dispositions depends upon the original qualifications, or capacities, of the organism and upon the stimuli it receives from its environment. Experience, in the wide sense of interaction between stimuli and organism and the results of this interaction in impressed dispositions, continuously enlarges or modifies capacity. The domain of stimuli, or of possible stimuli, increases with this alteration of capacity, for stimuli only become stimuli when they meet with an ability to receive them. An eyeless animal, for instance, or an infant before its experience entitles it to *see*, cannot perceive form or colour, though the same light can stimulate a trained human eye to perceive them. The four closely related terms 'memory,' 'disposition,' 'experience' and 'reaction,' vary together in meaning as the meaning of one of them is narrowed or widened; they also impose, as they expand or contract in meaning, a corresponding variation in the phrase 'qualifications or capacities of the organism.' 'Organic memory' implies (or may imply) organized dispositions to reactions impressed by interactions between

the organism and stimuli to which it is competent to respond in a total process of wholly unconscious experience.

'Memory' is usually restricted to a process formally similar to the above but including consciousness. The sight of a lyre recalls the person who last played upon it and the tune he played: the mind reacts consciously to a stimulus consciously received because conscious experience has impressed a disposition upon it to react in this conscious way. The essence of memory is disposition to react, organized through experience, whether that reaction be expressed in unconscious movement or in conscious reference to the past. Memory tends to be identified with conscious reference to past experience because, just because it is conscious, this conscious reference is striking and arrestive. Memories, conscious mental processes, are naturally accorded the rôle of constituent elements, regarded as composing memory as a whole and conceived away from their real nature of conscious reaction to appropriate stimulation of the organized disposition to react which constitutes memory as a whole. In its most explicit form, a memory recalls a past event with its date attached: a cyclist remembers, for instance, that he was caught in a storm last Tuesday. The dating is often less explicit or even absent, as in remembering that one had been ill after eating strawberries without specifically recollecting the time and place. These conscious references to the past are mental reactions depending on an organized disposition impressed by experience: the cyclist's wetting impresses on his mind a tendency to the reaction of consciously referring to his experience of getting wet. The reaction induced by memory usually involves no conscious reference to the past—we catch trains, put on our clothes, deduce causes from effects or effects from causes without, in any definitely conscious way, *remembering* the past experiences that determine our actions or thoughts. Underneath all conscious mental process and vitally continuous with it, lies a volume of unconscious process that takes its course and determines conscious reaction through its organization into dispositions during the individual's experience.

7. This unconscious process is as integral a part of any thought, idea or perception as the part that we realise consciously. The relation of the word to its meaning, with which this article is most particularly concerned, is, it may be suggested, appropriately represented by the relation between stimulus and stimulated process. Words have meanings because they set the mind going in a determinate way or because they strike into mental process already in being to direct its course. The concrete reality of mental life constantly escapes out of the sharply de-

fined concepts or logical distinctions employed to interpret it. The word may appear in the conscious region as a part of the total mental process or reaction: the sight of a horse, for example, may flash upon consciousness the word used to designate the animal. The extension of speech and writing among civilised peoples continuously extends and expands the functions of words in all mental process and justifies the concept of the word as a stimulus to total mental reaction. This conception of the word as a consciously applied stimulus and of its meaning as an unconscious reaction determined by an organized disposition impressed by experience seems to give a more adequate mental grip on the nature of ideas and their passage through the mind than the notion involved in the terms 'sign,' 'symbol,' or 'mark' that have so often been applied to words or names. According to this conception, an idea, of a horse, for example, is a sectional part of a mental process that occurs, to draw a distinction quite sharply for analytical purposes, quite unconsciously, according to a disposition organized by experience and directed by a word as a conscious stimulus. The word is not the only image that can provide the stimulus for the process of the idea: a visual image of a horse can replace the word 'horse'; but it is with words that this article is concerned.

8. Professor Bosanquet virtually equated 'word' with 'stimulus' when he described (in his *Logic*[1]) a name as a sign that rouses the mind to a set of activities with an identical element. Hobbes realised that the name, which he defined as "a word taken at pleasure to serve for a mark, which may raise in our mind a thought like to some thought we had before, and which being pronounced to others, may be to them a sign of what thought the speaker had or had not before his mind," operated as a stimulus, for he observes that 'marks,' which include words, *recall* thoughts "by the sense of them[2]." The terminology of 'signs' and 'marks,' however, tends to imply the erroneous notion that ideas exist, as such, in the mind in unconscious sojourn and are simply 'recalled' into consciousness by the words that remind the experient of them. Hobbes compares 'arbitrary' signs to the bush formerly hung up before a wine-shop to intimate that wine could be bought there. The bush and the wine-shop are perpetually joined because they are always there together. This naturally suggests that the idea, as also perhaps the word representing it, continuously exists as such—its existence nowadays being specifically referred to the unconscious. The same tendency to error is manifest in Locke's remark that "the use of words" is "to stand

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[1] *Logic*, 1888, I. 13.

[2] *Elements of Philosophy*, pt. 1, ch. ii.

as outward marks of our internal ideas[1]." Something permanent is implied in the permanence of meaning, but the permanent element is a disposition organized by experience for the mind to react in the form known as an idea to the stimulus applied by the word; it is not, such at all events is the view here maintained, a permanent existence of ideas that sojourn in the unconscious from whence they rise from time to time into the focus of consciousness or into its outskirts.

If the essence of mental life be process, it is difficult to conceive how a present idea, which is a particular movement in the mind, could be lodged continuously in the unconscious while other ideas, mental processes in their turn, are in progress. Psychical dispositions no doubt imply psychical structure, for function and process involve organs and arrangement of parts, but the structure developed in the mind growing under experience is not the simple absorption of sensations, perceptions or ideas, as they consciously occurred, and their introduction into it as bricks are built into a house. "Nevertheless," writes Professor Ward in his *Psychological Principles*, "it may be urged, it is surely incredible that all the incidents of a long life-time and all the items of knowledge of a well-stored mind, that may possibly recur—'the infinitely greater part of our spiritual treasures,' as Hamilton said—are severally retained and continuously presented in the form and order in which they were originally experienced or acquired." Professor Ward adds that "This, however, is not implied." It is doubtful, however, how far his "psychological principles" desert the belief that conscious images, perceptions and the like persist as such to leave and enter consciousness. He observes, for example, that "the same image may figure in very various connections," implying that "incidents of a long life-time and all the items of a well-stored mind" may be simply re-ordered or rearranged without losing their permanent, substantial individuality. "Because of the manifold forms into which they may *evolve*," he continues later on, "subconscious images, while still *involved*, are sometimes called 'psychical' or more definitely 'presentational dispositions.' The word *disposition* means primarily an arrangement...." He continues: "These dispositions are processes or functions more or less inhibited, and the inhibition is determined by their relation to other psychical processes or functions," a sentence which suggests that the organized dispositions to react, impressed by the process of experience upon the mind, do not imply that percepts and ideas are individually retained in the unconscious to rise from time to time into consciousness. When disposition is referred primarily to arrangement this is precisely what is suggested. Professor Ward's preference for the term 'subconscious,' which he seems to regard as equivalent to greatly reduced attention, together with his division of the mind into presentations and the subject to which they are presented, seems to incline towards the conception of Bleuler that a sensation may exist unconsciously exactly as it occurred consciously, except for its deprivation of conscious quality.

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[1] Essay 3, II. 9.

9. Professor Ward's way of thinking seems on the whole to favour the belief that when the visual image of a horse appears in a mind that has perceived the written word h-o-r-s-e, it simply moves from under a subconsciously attenuated attention to a position where it receives focal or concentrated attention. Whether the movement pertains to the image or to attention itself, or whether it be a joint relation that may conveniently be spoken of either as a movement of attention to image or as a movement of image under attention, does not affect the point here emphasized, the point, namely, that the image as it appears and appeared in consciousness is a permanent psychical lodger. There is a difference between this conception and supposing that the visual experiences of horses and the ideas consciously entertained regarding them impress a disposition upon the mind, organized in the region of unconscious memory, to react to the stimulus of the word by flashing the visual image upon consciousness. The hypothesis presented here is that the word is essentially a directive stimulus to which the organized dispositions within the unconscious respond by a definite process or reaction. This reaction may result in conscious process; it may be virtually completely unconscious, and it is always unconscious in part. When conscious processes do result, they are temporary presentations proceeding from a total process originating in organized dispositions: they are not passages, as when the visual image of a horse is received consciously, of permanent elements from the unconscious to the conscious. The recurrence in memory of an incident formerly perceived is a psychical reaction similar to, but by no means identical with, the psychical reaction in the original perception. The percept does not sojourn in the unconscious or remain under subconsciously attenuated attention; it impresses the mind and memory is organized into a disposition in the unconscious region to react under suitable stimulation by producing a conscious process that contains a reference to the original impression.

10. Words are frequently described as 'expressive signs' and contrasted with 'suggestive' and 'substitute' signs. "A word," writes

Professor Stout[1], "is an instrument for thinking about the meaning which it expresses, a substitute sign is a means of not thinking about the meaning which it symbolises." The symbols of algebra, arithmetic and formal logic are largely mere substitute signs that are substituted for their meanings, as counters may be substituted for coins during a game of cards, and operated upon according to fixed and definite rules derived from the nature of the things symbolised. Expressive signs, such as words, are used for the sake of their meaning, substitute signs are used instead of their meaning. There are also signs that are merely suggestive because they have no meaning. Professor Stout calls the shape of the knight in chess, or the chalk-mark on Ali Baba's door, "a mnemonic help," a device for calling up a certain idea which is then exclusively attended to, the suggestive sign being dropped when it has done its mnemonic duty. The extraordinary conclusion that a chalk-mark *made for a specific purpose* has no *meaning* indicates pretty plainly that the analysis is defective. John Stuart Mill[2] denied meaning to the proper name because it did no more than the chalk-mark, and it is obvious that the doctrine of 'suggestive signs,' as expounded by Stout, will naturally lead to this denial.

These distinctions between expressive, suggestive and substitute signs would not have been so sharply drawn if it had been realised that the 'sign' functions essentially as a directive stimulus. All signs direct mental process in a *definite* way—if they did not do this they would not be signs. Any object may suggest one thing to *A* at one moment, another thing at another moment and every individual person may receive from this same object a different suggestion. A cow may suggest either milk or a dairymaid to one person, an item of religion to an Egyptologist, the Indian Mutiny to an historian and a dozen other things besides. If the model of a cow were hung up over every dairy, it would be *tied* to the associated idea 'this is a dairy' and become a sign. It would then *mean* that milk was sold within, just as the bush, formerly hung over wine-shops, which might suggest any idea while on its native heath, acquired a definite meaning as a sign by becoming tied to the intimation that wine could be bought there. Where there is tied association there is meaning, and where there is determinate direction of mental process, which is the same thing under a different aspect, there is meaning also. A word has meaning because it stimulates organized psychical dispositions to determinate reaction; the shape of chessmen and the robber chief's chalk-mark have a precisely similar function. The substitute sign also determines mental process in a determinate way, for it would be useless if it left the mind free to think exactly as it chose.

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[1] *Analytic Psychology*, 1896, II. 194.

[2] *System of Logic*, 10th ed. 1879, I. 37.

11. Many psychologists think like Professor Stout that the word "is an instrument for thinking about the meaning which it expresses." Professor Hoernle, in an article on Image, Idea and Meaning[1], says that the meaning normally occupies the focus of attention: the word is, of course, one species of image. But, if these two statements can be regarded as equivalents, do they not apply, if they apply at all, to the signs regarded as merely suggestive? 'The horse will gradually become extinct as motor travel and traction develop': does the string of words not run through the focus of attention, definitely stimulating and controlling a mental process that virtually constitutes a wholly unconscious process? If a definition of any word be demanded, attention may be said, in a sense, to turn towards its meaning. In defining the word 'horse,' for example, the dispositions unconsciously organized in memory respond to stimulation by processes that result in conscious realisations that a horse is an animal, that it eats oats, that it has four legs etc. Insistence on conscious realisation of the total unconscious psychical dispositions involved in the comprehensive idea of a horse would detain the mind and prevent the continuous mental flow that responds to the whole sentence. It seems to be essentially true of the expressive word that it avoids the intrusion of meaning into consciousness by appropriating the rôle of a definitely directing stimulus on the whole mental process comprising unconscious working and consciously experienced image. Translated into terms of attention this means that it is the word, or image, that receives attention and not the meaning. We think with a word, when we have mastered its definition, without the constant obtrusion of that definition. The value of the word for thinking, privately indulged or publicly expressed, lies in the ease and effectiveness with which it guides the whole mental process without mental reactions of a conscious character. The 'suggestive sign' operates in a way very similar to the demand for a definition of the expressive sign or word. *A* writes a letter at night that he must remember to post in the morning. His desire produces a disposition to think of posting the letter and this disposition remains during the night in his unconscious mind. To stimulate this disposition into so reacting that he will consciously think of the act of posting, he ties a knot on his handkerchief. When he wakes in the morning his mental attitude to the knot may be said, without serious error, to be an attempt to define its meaning. The meaning of

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[1] *Mind*, N.S. XVI. 82.

the knot is temporary, it may mean on the succeeding night that *A* must pay an account for his wife: this is no reason for denying meaning to the suggestive sign, though this temporariness of meaning probably induced Professor Whitehead[1] to oppose the knotted handkerchief, as a merely suggestive sign without meaning, to the word with a permanent meaning, as an expressive sign. A sign is a directive stimulus to mental process: it may, as an expressive word, stimulate a process that reacts unconsciously; it may, as a suggestive sign, stimulate organized psychical dispositions into producing a conscious reaction. A word, taken out of verbal context, may function similarly to a suggestive sign by stimulating to the conscious reactions involved in definition or actually become one by consciously referring the mind to the experiences implied in the organized mass of unconscious disposition that constitutes its meaning.

The 'substitute sign' stimulates now the mental process represented by the meaning it is to express when the final calculation is made and now the mental process whose meaning is connected with the mathematical operations to which it is submitted. But all signs, like all words, are stimuli directive of mental processes that proceed largely unconsciously, like the organized dispositions responsible for them, and these mental processes, or reactions, may, more or less, according to circumstances, be conscious operations.

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[1] *A Treatise on Universal Algebra*, 1898, I 3.

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THE RÔLE OF INTERFERENCE FACTORS IN PRODUCING CORRELATION.

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1. *Introduction.*
2. *Possible mechanisms producing correlation.*
3. *Terminology.*
4. *Condition for the existence of general and specific factors in the case of three variates with positive correlation.*
5. *Weldon's experiment and negative correlation.*
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7. *The effect of 'interference' in the case of three variates.*
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1. *Introduction.*

THE object of this paper is to make some advance in our knowledge of the significance of the correlation coefficient. In setting out the numerical results of psychological and pedagogical experiments this coefficient is so largely used, and by its aid conclusions are so freely suggested, that it becomes of increasing importance to examine with great caution the nature of the deductions which can be made from the magnitudes of the coefficient. While a study of correlation is essentially general to all scientific measurements, the particular problem before us here is the use of the coefficient in attempting some analysis of ability in mental processes. In one sense the significance of the correlation coefficient is very well understood, viz. in the sense of the definition of Galton. If an array of individuals is selected from the population, all having a certain value for one of two correlated variates, then the average value which these individuals show for the other variate will be '*r*' times the former value, provided that all measurements are made from their respective means and in the appropriate units, which are their respective standard deviations. This Galtonian point of view therefore supplies us with a very definite and illuminating meaning for the correlation coefficient: if a large number of pairs of measurements are made for each of two variates (if, *e.g.*, the behaviour of a large number of individuals is recorded in response to two distinct mental tests), each individual thus giving a pair of measurements, we then have two arrays of values showing a more or less intimate relation, and the correlation coefficient is an exact, though not the only, measure of the intimacy of this relation. While the correlation may thus be clearly measured and expressed as a fraction, and while no doubt remains as to the meaning of the fraction as seen from the Galtonian point of view, yet the cause of that relation between the two arrays—which are the factors that decide, and in what way they inter-act to bring about that relation—is left at any rate without a direct answer. This is the ground which needs exploration, to assist the interpretation of psychological and other data.

The two measured quantities are in general each of unknown complexity and we may speak as though some mechanism were at work producing the correlation. Consider, for example, two mental tests used by Burt[1]. (*A*) Tapping, with a blunt needle, and (*B*) dotting, using McDougall's dotting apparatus. The coefficient of correlation between *A* and *B* was ·48 with one group of children. According to a well-known theory, this result can be accounted for by assuming the presence of specific elements peculiar to tests *A* and *B* respectively, and, in addition, common elements operating similarly in each. There are many other possible explanations, however; one other in particular, which this paper investigates, is that a number of elements is conceivable which operate in favour of one test and against the other. A group of such elements is described in this paper as an 'interference factor'; it has been already foreshadowed by previous writers[2]. The difficulty of the first-mentioned theory is that we are compelled to assume a large number of specific elements to counteract the high correlating influence of the common elements, whereas, as will be shown, the assumption of an interference factor, helping one test while hindering the other, readily accounts for lowness of correlation between two very similar tests, without the assumption of numerous specific elements. It will also be shown that the assumption of interference may throw light on other psychological difficulties.

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[1] This *Journal*, 1909, III. 132, 153.

[2] See page 91.

2. *Possible mechanisms producing correlation.*

What are the conceivable causes of correlation between two measurable phenomena? The most easily conceivable is that there are some actual causes which are the part causes of each of the phenomena. The simplest illustration of this is afforded by a generalised form of Weldon's experiment[1].

This is an easy dice experiment designed to give two arrays of numerical values, such as those referred to in the foregoing section, except that the mechanism of correlation is decided upon, and that the variations are known to be solely dependent upon the laws of chance as they apply to dice throws. For the sake of distinction let some dice be coloured red, the others being white. One pair of values, being respectively the measures of two correlated phenomena, is found as follows. Throw a number of dice, say l red dice and m white dice, and note the total score, which is one of the required values. Now let the l red dice remain untouched, remove the m white dice and throw in their place n white dice. Again take the total score of red and white dice, which is the other required value. Any number of pairs of values can be found in the same way, thus making two arrays of correlated numbers. Obviously the source of correlation lies in the fact that a certain number (l) of red dice is always a common factor in a pair of throws. The correlation coefficient r may be calculated from the 'product moment' formula, but it is possible to prove without performing the experiment at all that r may be calculated from the equation

$$r = \frac{l}{\sqrt{(m+l)(l+n)}} \qquad \text{......................(1).}$$

If the two groups of dice are numerically equal, *i.e.*, if $m = n$, then r is simply the proportion of red dice among the total number used. Or if we let the red dice alone form the second throw every time, then $n = 0$, and

$$r^2 = \frac{l}{m+l} \qquad \text{..................................(2).}$$

The illustration treats the elemental causes, the single dice, as additive. It may be asked what would happen were they to combine in some other way, such as by multiplying. It can easily be deduced from Bravais that the results are the same, provided that the form of the function according to which the elements combine is the same in each variate, and the standard deviation of each element is the same.

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[1] Thomson, This *Journal*, 1916, VIII. 274.

In the above instances the correlation has been due to actual overlapping, and many real cases of correlation may be due to such. But there are other forms of correlation conceivable in which the connexion is much less tangible. The best example on the same lines as that of overlapping dice throws is perhaps correlation in whist hands. At whist, the number of trumps in my hand is negatively correlated with those in my partner's; if I have many trumps, he has probably few, and *vice versa.* There is similarly a positive correlation, where the connexion is still less direct, between the number of cards of one suit in my hand and the number of cards of any other suit in my partner's. In these cases there is no clear physical overlap, nothing which can be visualised (like the red dice) as being the cause of the correlation. Instead of overlapping, we have here exclusion due to drawing from a limited pool. If I have many red cards dealt to me, there remain proportionally more black ones which may be dealt to my partner. There is a natural tendency to think of correlation in terms of overlapping factors instead of in terms of exclusion as in whist: but it must always be remembered that from the mere presence of correlation we cannot deduce that any such actual overlapping of the phenomena occurs.

In the present paper the simplest case only is to be considered, viz. that in which the correlations can all be considered to be formed in a way generalised from Weldon's experiment; that is to say, the variates are to be formed by the simple addition or subtraction of factors, and all the correlations are to be considered as due to the fact that some of the factors are added to, or subtracted from, more than one variate. Throughout it is necessary to keep in view this limitation, and to avoid any temptation to draw conclusions of too wide a nature. Other forms of correlation-producing mechanisms will be considered on some future occasion.

3. *Terminology.*

It appears necessary, in order to avoid misunderstanding, for us to state clearly the meaning of the terms 'specific,' 'group,' and 'general' as used in this paper. By general factor is meant a factor which is common to all the variates under consideration. A group factor is a factor common to some but not to all variates under consideration. The word 'specific' is used for a factor which occurs in only one variate among the variates considered. The question whether a factor is to be called specific, group or general depends upon the selection of variates for consideration. A particular factor may be specific in one selection of variates, *i.e.* operating in only one of them, but it can be a group factor if the selection of variates contains two or more variates to which this particular factor is common; and if such a selection of variates be made in which this particular factor is common to all, it can become a general factor.

The expression 'general factor' is sometimes used in a different sense[1] from the above, *i.e.* to signify a factor common to all possible mental operations, a meaning which is not intended in this paper. The word 'specific' is also used by some writers[2] to indicate a factor which is common to a limited number of variates. Such a factor would obviously be called a group factor in this paper. A further distinction in the use of the word general is given later (footnote, p. 93).

4. *Condition for the existence of general and specific factors in the case of three variates with positive correlation.*

The extension of Weldon's experiment to three variates[3] x, y, and z involves the use of seven groups of dice, viz. white dice which are common to all three variates; orange, green, and purple dice, which are common to x and y, y and z, z and x variates respectively; red, yellow, and blue dice which are peculiar to the x, y, and z variates respectively. Let the seven groups of dice be visualised as forming a pattern of seven cells, each cell containing all the dice of its proper colour, for instance W cell contains white dice, and so on. The cells are arranged for convenience as in Fig. 1. Let R, O, Y, G, etc. be the actual numbers of red, orange, yellow, green, etc. dice respectively.

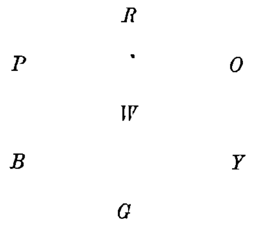

Fig. 1.

Instead of two arrays of numbers as in section 2, three arrays can now be made with overlapping correlations between any pair of the three. A trio of values for the three variates will be found as follows. Throw all the dice and note the total score of each group The sum of the scores of the P, R, O, and W groups will give a value for the x variate; the sum of the scores of the O, Y, G, and W groups will give a simultaneous value for the y variate; and a sum of the scores of the G, B, P, and W groups will give a simultaneous value for the z variate. A number of repetitions of such throws will give three arrays of values for the three variates respectively.

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[1] Cf. Hart and Spearman, This *Journal*, 1912, v 58.

[2] Cf. N. Carey, This *Journal*, 1915, viii. 78.

[3] G. H. Thomson, This *Journal*, 1919, ix. 325.

The correlation between the variates x and y will be

$$r_{xy} = \frac{W+O}{\sqrt{(R+O+W+P)(Y+G+W+O)}} \quad \text{.........(3).}$$

r_{yz} and r_{zx} can be found from similar equations. W is here called a general factor; P, O, and G, group factors; R, Y, and B, specific factors. Since R, O, and Y, etc. represent actual numbers of dice, these numbers are naturally treated as positive, and so far all that has been said applies to positive cells. It should be noted that only group and general factors appear in the numerators of the three coefficients, while all factors appear somewhere in the denominators. If, for instance, the value of R is increased, both r_{xy} and r_{zx} will be diminished, and r_{yz} remains unchanged. If, however, W is increased, all three coefficients will be increased, and similarly a fall in W will be reflected by a diminution of all three coefficients[1].

It appears that, when measurement reveals a set of high correlations among three variates, there is a favourable chance that a general factor W underlies these results; and that, when correlations are low, the chance of a general factor becomes more unfavourable. To make the problem more definite we can inquire whether or not there is a magnitude to which the r's can rise, above which we can say with certainty that a general factor exists, or, if it is known not to exist, that the correlations must have been produced in some way different from that here contemplated.

It is obvious, on the other hand, when a set of correlations are very low, that we cannot say with certainty that a general factor is *not* present, for such correlations, however feeble, *may* have been produced by such a general factor. If we strive to create patterns of ever-increasing correlations without the aid of a general factor, our only course is to increase the group factors in comparison with the specific factors. This process cannot be continued indefinitely, as even infinitely large group factors will not produce complete correlation. The same limit is reached when the specific factors are reduced to zero and the group factors remain. In such a case the group factors hold the field, and we have reached the highest set of correlations possible without calling in the aid of a general factor.

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[1] In practice, in psychological or other problems, the facts concerning the separate elements P. R, W, etc. are not known. It is necessary, therefore, to observe the greatest caution in making any such inferences as to the nature of the underlying mechanism producing the correlations. Indeed, the incentive to the prosecution of such studies as the present is the belief that a great many such inferences have, in the past, been made without that caution, and without stating what assumptions underlie them.

It can be shown[1] that in this case

$$r^2_{xy} + r^2_{yz} + r^2_{zx} + 2r_{xy}\,r_{yz}\,r_{zx} = 1 \quad \ldots\ldots\ldots\ldots\ldots(4).$$

This expression marks the boundary between values of r_{xy}, r_{yz}, and r_{zx} for which a general factor is necessary, and those for which it is unnecessary although possibly present. The proof already published shows how this expression, which we shall call D, was arrived at. Its properties may however be seen more clearly from the following treatment, which also gives an independent proof.

We have

$$\begin{aligned} D &= r^2_{xy} + r^2_{yz} + r^2_{zx} + 2r_{xy}r_{yz}r_{zx} \\ &= \frac{(O+W)}{(P+O+W+R)(O+G+W+Y)} \\ &\quad + \frac{(G+W)}{(O+G+W+Y)(G+P+W+B)} \\ &\quad + \frac{(P+W)}{(G+P+W+B)(P+O+W+R)} \\ &\quad + \frac{2(O+W)(G+W)(P+W)}{(P+O+W+R)(O+G+W+Y)(G+P+W+B)}. \end{aligned}$$

After algebraic simplification this expression reduces to the form

$$\frac{A + W(K)}{A + B(K') + R(K'') + Y(K''')} \quad \ldots\ldots\ldots\ldots\ldots\ldots(5),$$

where K involves only general and group factors, and all quantities have positive values.

From the expression in this form the following conclusions can be drawn:

(1) If $D > 1$, the general factor W must have a real positive value.

(2) If $D < 1$, the specific factors R, Y and B cannot all be zero. (If $R = Y = B =$ zero, then the denominator of expression (5) reduces to A only.)

(3) The effect of increasing any specific factor is to reduce D.

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[1] See the joint appendix to an article by G. H. Thomson in This *Journal*, 1919, IX. 335.

(4) If the general factor W = zero, D cannot be greater than unity.

(5) If the general factor W and the specific factors R, Y, B are all zero, D must be unity.

These conclusions have already been deduced from general considerations[1], and their application may be shown by reference to the following purely illustrative examples.

(1) Experiments in Auditory Memory[2].

x variate = Pitch of musical notes, y variate = Rates of ticking,
z variate = Musical phrases.

$$r_{xy} = \cdot 36, \quad r_{yz} = \cdot 36, \quad r_{zx} = \cdot 55, \quad D = \cdot 704.$$

(2) Experiments in Verbal Memory[3].

x variate = Auditory Motor, y variate = Visual,
z variate = Auditory.

$$r_{xy} = \cdot 52, \quad r_{yz} = \cdot 82, \quad r_{zx} = \cdot 76, \quad D = 2\cdot 168.$$

The contrast between these two sets of memory experiments is significant, D being less than unity in one case and greater than unity in the other. Dice patterns on the basis of Fig. 1 may be made with or without a general factor in the first case, but must have a general factor in the second; whereas specific factors can be dispensed with in the second case but not in the first. In order to find what can be regarded as a high value for D, consider the results which would be obtained were we to apply the *same* mental test three times running (as we do in measuring reliability coefficients), instead of three *different* tests. Taking ·7 as a fairly common reliability coefficient, this would give $D = 2\cdot 156$. Beyond this value D rapidly increases towards 5, which is its maximum value.

It is necessary to emphasize that these inferences from expression (5) only apply to a mechanism with the conditions imposed. The inferences respecting specific factors will be considerably modified when a source of negative correlation is introduced. It would be still further unsafe to claim validity for these inferences in mental measurements when both the elements and the mechanism of correlation are unknown, but it may be possible in such cases to *disprove* general statements, that is disprove their generality, with the aid of the above inferences.

At this stage it is necessary to indicate a method of substituting numerical values for the symbols in Fig. 1. The method is described in Appendix I, but we must note here that only positive numerical values can be allowed for the cells. Why is this limitation of positive cells imposed on Fig. 1? Suppose for example the value of R is negative, understanding this to mean that the scores of the red dice are to be subtracted instead of added in computing the variates. This would reduce the variate x *but the values of* r_{xy} *and* r_{zx} *would be just the same* as if R were positive. It can also be seen in the case of group factors that a negative P for instance while tending to reduce equally both variates x and z gives the same value for the r's as when P has a positive value. A similar statement is also true for W, the general factor[1].

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[1] This *Journal*, 1919, IX. 336.

[2] N. Carey, *ibid.* 1915, VIII 79.

[3] *Ibid.* 1915, VIII. 83.

Thus, when the only data we possess are the correlation coefficients themselves, it is impossible to say whether positive or negative terms occur in the pattern. No further purpose is therefore served in this paper by introducing negative cells. In applying equation (1) to negative values, a mere algebraic substitution of these negative values is not permissible. Equation (1) is only concerned with the *numerical* values of l, m, and n and is independent of their algebraic sign.

The problem of negative correlation, however, is quite another matter and is now to be dealt with.

5. *Weldon's experiment and negative correlation.*

The simple form of Weldon's experiment has already been described. The question arises whether negative correlation can be visualised in some simple way by a modification of Weldon's experiment. This can readily be accomplished by allowing the score of the common dice to be considered positive in the first variate and negative in the second or *vice versa*. In both cases

$$r_{xv} = \frac{-l}{\sqrt{(m+l)(n+l)}} \quad \text{.........................(6).}$$

Such a set of common dice can now be regarded as a source of negative correlation—not the only possible source by any means, but one that can readily be visualised, and that lends itself to mathematical analysis. It may be of assistance in interpreting correlation data.

Having secured a method of producing both positive and negative correlations by dice throws, it is possible to imagine two sources of correlation operating together, the one tending to produce positive and the other negative correlation. Such would be the case if there were two common sets of dice, l_1 and l_2, where the l_1 dice are regarded as positive

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[1] For proof of these statements see Appendix II.

(or negative) in both variates, and the l_2 dice as positive in one variate and negative in the other. We then have the modified formula

$$r_{xy} = \frac{l_1 - l_2}{\sqrt{(m + l_1 + l_2)(n + l_1 + l_2)}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(7),$$

involving four quantities m, n, l_1 and l_2. r will therefore be positive or negative according as l_1 or l_2 is the greater, and will be zero when $l_1 = l_2$. It is even possible to produce all correlations between $+1$ and -1, by these dice experiments, *without the use of specific factors at all*, in which case we have

$$r_{xy} = \frac{l_1 - l_2}{l_1 + l_2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(8).$$

A proof of equations (6), (7) and (8) will be found in Appendix II.

6. *The effect of 'interference' in the case of two variates.*

An examination of equation (7) reveals the fact that the magnitude of the correlation between two variates gives no criterion as to the magnitude (not even the comparative magnitude) of the common factors involved. Correlation coefficients are sometimes held to give information in this respect. Take, for example, the following statement[1]: "Zero correlation always gives information concerning both (*i.e.* general or group) factors, that is, that they are both non-existent" We see by equation (7) that two variates may have zero correlation and yet the common factors l_1 and l_2 may be very large, so long as they are equal. Very low correlations are not inconsistent with very high common factors. To make statements involving the values of the unknown factors on the sole evidence of the correlation between the two variates is to take for granted the solution of a single equation involving more than one unknown quantity. The correlation coefficient of itself gives no certain evidence concerning the size of common factors.

That difficulty has been felt in this direction is evident from the writings of W. H. Winch and W. G. Sleight on the question of transfer. Winch says "It appears...that very low and indeed doubtful positive correlations between two mental functions ..., may nevertheless be consistent with some real connection between those functions within the same mind[2]." Sleight says "It is evident that the old *a priori* methods are useless in the treatment of such problems; and further, that it is equally impossible to frame any such general law as that of the 'common element'...The results before us prove so complicated that each case needs special investigation[1]."

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[1] N. Carey, This *Journal*, 1915, VIII. 73.

[2] This *Journal*, 1910, III. 406.

As regards the question of transfer of improvement the interest in the present paper turns on the common factor which reduces positive correlation by tending always to produce negative correlation, viz. the factor represented by l_2 in equation (7). In dice experiments this factor is regarded as adding its score to one (say the x) variate, and subtracting the same score from the other (the y) variate. Such a factor we have conveniently called an 'interference factor' as it increases the scores of one variate and decreases the scores of the other. As a very simple device illustrating a source of negative correlation, a rôle which it performs to perfection in dice experiments, the interference factor deserves notice and serves as a valuable method of testing conclusions which are obtained from correlation data. The idea of interference in mental tests is not new. Sleight[2] says "Differences in mental processes may lead to loss of transfer or even reciprocal interference." In another connexion we find the following: "The process of learning continues after it ceases to be conscious and this unconscious continuance is greatly interfered with by any further mental work[3]."

The recognition of the interference factor throws some light on the complicated and diverse evidence obtained from mental tests, which are used for determining both improvements and correlations. For the sake of clearness and brevity let us take a case from dice experiments using equation (7).

Let $m = 5$, $n = 5$, $l_1 = 10$, $l_2 = 5$, where l_2 is positive for x.

Then by simple arithmetical calculation:

$$r_{xy} = \cdot 25.$$

Average score of $x = 70$ with a possible range of 100.
" " $y = 35$ " " 100.

Now let improvement take place, and let improvement be simply represented by increasing the number of dice. The following are three cases of improvement and their consequent effects:

Case I. Let improvement in x be represented by the addition of one die to m, l_1, and l_2.

We then have $r = \cdot 22$.

Average score of $x = 80\cdot 5$ with a possible range of 115.
" " $y = 35$ " " " 110.

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[1] This *Journal*, 1911, IV. 423.

[2] *Op. cit* 386.

[3] Hart and Spearman, This *Journal*, 1912, v. 70.

Here correlation is decreased; average value for y is unaltered but its possible range is increased.

Case II. Let improvement in x have the large and proportional effect of doubling m, l_1 and l_2.

We then have $r = \cdot 267$.

Average score of $x = 140$ with a possible range of 200.

„ „ $y = 52{\cdot}5$ „ „ 175.

Here correlation is but slightly increased and there is only half the ratio of increase in y compared with x, but three-quarters of ratio of increase of possible range.

Case III. Let both x and y be separately improved, adding one die to each factor, thus causing excess improvement in l_1. Let increased values be $m = 6$, $n = 6$, $l_1 = 12$, $l_2 = 6$.

We then have $r = \cdot 25$.

Average score of $x = 84$ with a possible range of 120.

„ „ $y = 42$ „ „ „ 120.

Here correlation is unchanged by such improvement.

In the three cases of improvement given above, the ordinary assumption of transfer through common elements is used. An exhaustive study is not here intended, the three cases being selected as suggestive illustrations. Something of this nature should be borne in mind when endeavouring to interpret the results of experiments in mental measurement. Case II contains increases of the original values in proportion to their original magnitudes, while Cases I and III do not follow such proportion. It is impossible to lay down a law of improvements in mental operations themselves, and hence these dice representations of improvement do not contradict any known principles.

In Case I, although transfer is assumed, there is no increase in the value of y, but there is an increase of scatter, and further in spite of an improvement in common factors the correlation is decreased. The effect of improvement in the y variate is used up in balancing the interference effect and no improvement is noticeable as a result. Does the absence of improvement in the second variate then refute the doctrine of transfer through common elements? Not necessarily, for Case I assumes transfer and yet no improvement takes place in the y variate, and this again is accompanied by a reduction in correlation. Case II might occasion no surprise so far as the improvements are concerned; and yet in spite of the large improvement in the common factor, the correlation coefficient is but slightly increased compared with the usual errors of experimental measurement. In Case III although both variates are separately improved and the common factor l receives, therefore, a double impetus, yet the correlation coefficient remains unaltered. The contemporary training of two abilities with common factors does not necessarily alter their correlation.

It is not surprising therefore that the results of experiment in transfer show great diversity, and we do not yet know sufficient about the factors involved, to say whether the results support or refute the doctrine of transfer. Correlation data do not yet give us the desired knowledge about the factors involved in mental processes. A combined transfer and correlation research would be valuable, if properly planned, and the writer hopes to carry out such an experimental inquiry in the near future.

7. *The effect of 'interference' in the case of three variates.*

We now turn our attention to the effect of interference in the intercorrelations between three variates. It has been found necessary to distinguish between two classes of correlation-producing elements, according as they tend to produce positive or negative correlations. This distinction may be used in the seven-fold pattern (see Fig. 1) and will have the effect of dividing the group factors each into two parts, the first indicating the number of dice whose score is added to (or subtracted from) both its variates, and the second indicating the dice whose score is added to one variate and subtracted from the other. Let these two parts of the group factor be denoted by the suffixes 1 and 2 respectively, see Fig. 2. The modification of the general factor for three variates is complicated by the fact that a factor may add its score to one variate and subtract it from the other two, thus becoming a source of positive correlation between one pair of variates, and of negative correlation between the other two pairs. This may be met for instance by using a factor W_x, where the suffix x signifies that the score is added to the x variate and subtracted from the y and z variates. Let W_y and W_z have similar meanings with respect to the variates y and z. For uniformity let W_1 indicate a general factor which produces positive correlations only[1].

The whole may be visualised in Fig. 2 which is a modified form of Fig. 1. It represents a dice pattern for three variates showing sources of correlations on the principles described, by imposing interference factors of a group and a general nature.

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[1] Clearly, in ordinary parlance only W_1 would be called the general factor. The others, W_x, W_y and W_z are general in the sense that they have an influence in each variate: but that influence is not always in the same direction, in some it is beneficial and in others it is harmful. Care must be taken to preserve a careful distinction between these two kinds of generality.

In constructing numerical examples of Fig. 2 the method adopted has been to begin with a numerical pattern with no interference. Then arbitrary interference factors are introduced, care being taken to preserve the original correlations by leaving numerators and denominators of the correlation coefficients unaltered. This has in the main the effect of reducing the specific factors, in some cases even to zero.

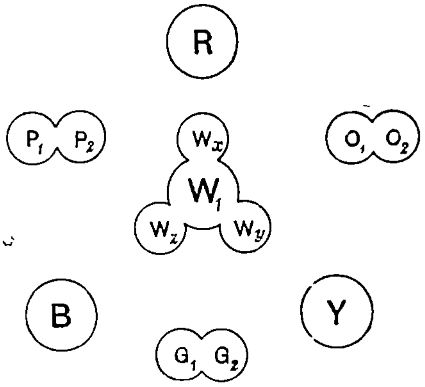

Fig. 2.

Although by the use of interference factors Fig. 2 still further complicates the mechanism of correlations between three variates, it must be remembered that correlation data may give rise to many interpretations, and that the aim is here to open up the way to different interpretations and to guard against the acceptance of any one interpretation by mere neglect of others.

8. *Applications to experimental results.*

The following is an example of experimental results and will be used to illustrate the great variety of possible interpretations. The results are taken from those of N. Carey[1]. Let three tests for visual, auditory, and verbal memory be tests for the three variates x, y and z respectively. We have $r_{xy} = \cdot 44$, $r_{yz} = \cdot 33$, $r_{zx} = \cdot 28$. We desire to use these coefficients in such a way as to make some conclusions about the factors involved in the three mental abilities. What information can these numbers give us?

We may proceed by asking what possible patterns of the dice experiments described will give these coefficients. A little trial soon shows that very many such patterns are possible, and to show their variety a few special cases are given in Fig. 3.

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[1] This *Journal*, 1915, VIII. 88.

Figs. 3 (*a*) and 3 (*b*) contain no interference factors. In (*a*) group factors are missing and in (*b*) the general factor is missing (this case is possible because $D < 1$, see page 87). So far then, neither general nor group factors are certain. Are specific factors certain? When interference is present, specific factors are not necessarily present, in spite of the fact that D is less than unity. Fig. 3 (*c*) is given to show that group factors alone without specific factors and without a general factor will give the required correlations. Fig. 3 (*d*) serves to show that these same coefficients may be produced by general factors only. Indeed general factors, including interference, can be constructed to give any set of three coefficients.

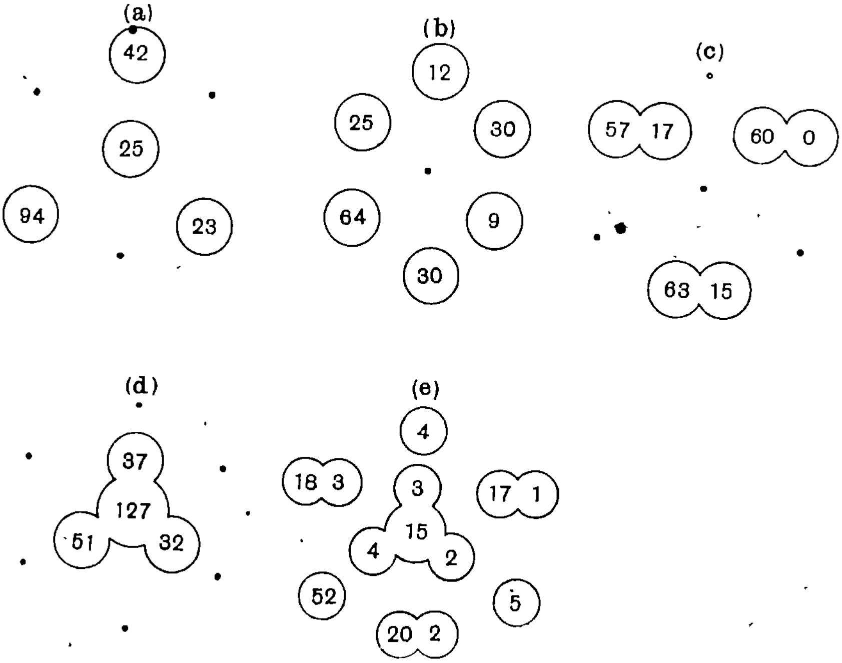

Fig. 3.

The correlation coefficients of themselves, therefore, do not constitute any certain evidence as to the factors involved. As we have seen, any factor may be dispensed with when D is less than unity; and when D is known to be greater than unity, all that can be said is that a general factor of unknown magnitude is present. There is no case, which cannot be realised without the use of specific factors, for these are found to be unessential among three variates, as also they were found to be unessential in the case of two variates. Since mental processes, however, are thought to be very complex, we may say that perhaps all kinds of factors are present in a greater or less degree. Fig. 3 (*e*) is given as an example of a pattern where all thirteen factors, that have been employed in dice patterns, are present. These factors constitute too many unknowns to allow of their determination from three correlation coefficients.

The failure to appreciate the large variety of ways in which correlations can be produced has led to a number of conclusions in psychological and pedagogical experiments that are not necessarily true; in fact, in some cases only one among the many possible interpretations is taken into consideration. Another set of results by N. Carey[1] will serve as an illustration

Six sensory discrimination tests are used, three of which are auditory tests, and three visual. The auditory tests are named Pitch, Rate, and Music, which we will call the x, y, and z variates respectively

We have $r_{xy} = \cdot 71$, $r_{yz} = \cdot 60$, $r_{zx} = \cdot 64$.

The visual tests are named Yellow, Angles, and Patterns, and if these are taken as a second set of x, y, and z variates, we have

$$r_{xy} = \cdot 13, \quad r_{yz} = \cdot 00, \quad r_{zx} = \cdot 05.$$

From these two sets of correlations the conclusion is drawn that "An auditory factor evidently has a wider range than a visual[2]." The expression 'wider range' is vague. It cannot mean that the auditory factor is common to a greater number of abilities for the experiments are not designed to test this. It may mean that the auditory factor may reach values greater by comparison than the visual factor does. This is certainly a possible interpretation of the results but it would be rash to rely upon it as the correct interpretation. If the test of the seven-fold pattern is applied, other interpretations are found to be possible. Patterns which would support the author's conclusion may readily be constructed as in Figs. 4 (*a*) and 4 (*b*).

The general auditory factor is here 60 while the general visual factor is zero. It cannot be argued, however, that the auditory factor is necessarily larger than the visual, nor is it of much use to say that it is probably larger so long as the probabilities are unknown. Fig. 4 (*c*) gives the same correlations as Fig. 4 (*b*) with a general factor of 80. It is quite possible to say, with the data in hand, that the general visual factor is greater than the general auditory factor[1]. All three patterns have intentionally been made *without specific factors*, so as to emphasize their irrelevancy in the present argument, and to remind the reader that they can be dispensed with, so far as interpreting correlation data is concerned. No special importance is to be attached to the actual numbers in the patterns. Countless patterns could be made, but these three are chosen as illustrations.

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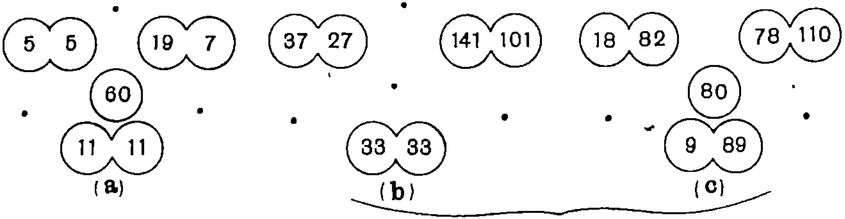
[1] This *Journal*, 1915, VIII. 78.

[2] The writer quoted uses the term 'specific auditory factor' to mean a factor common to the three auditory tests In the terminology of this paper such a factor would often be called a group factor, but in the present instance of three auditory tests it is called the general factor.



Auditory discrimination

Visual discrimination

Fig. 4.

9. *Summary*.

Among the many possible mechanisms producing correlations that by overlap of common elements presents a ready means of testing conclusions arrived at from correlation data. Even in the case of dice experiments very uncertain evidence as to the underlying elements is given by correlation coefficients, in view of the great variety of possible conclusions that may be drawn, and of the impossibility of saying which is the correct conclusion. Still more difficult is it to draw conclusions concerning mental abilities in psychological and pedagogical experiments when the method by which mental abilities produce correlation is unknown.

When the mechanism of correlation is one of overlap, as in dice patterns, and three variates are considered, a condition is, however, known which, when fulfilled, gives certain evidence of the presence of a general factor.

A simple modification of Weldon's experiment introduces a source of negative correlation, which either reduces the positive correlation present, or produces actual negative correlation. This phenomenon, here described as 'interference,' throws some light on the problem of transfer and suggests a possible line of explanation of the complicated and apparently inconsistent results obtained in actual experiment.

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[1] It is even possible to produce these three correlations (visual discrimination) by using general factors alone, as is done for another case in Fig. 3 (*d*).

Specific factors are never necessary in accounting for correlation results, while factors of a general nature are sufficient to account for any set of correlation coefficients, provided that these general factors include interference elements[1].

In conclusion, I wish to express my deepest thanks to Dr Godfrey H. Thomson for suggesting this investigation and for his frequent advice during its progress.

APPENDIX I.

The General Pattern for Three Variates correlated by Positive Overlap.

It is required to insert positive values in the cells of Fig. 1 so as to give three required values for r_{xy}, r_{yz}, and r_{zx} respectively. The method can readily be seen in the general pattern of Fig. 5, where the coefficients C_1, C_2, C_3 are equal to

$$\frac{r_{yz}}{r_{xy}r_{zx}}, \quad \frac{r_{zx}}{r_{xy}r_{yz}}, \quad \frac{r_{xy}}{r_{yz}r_{zx}},$$

and m, n, p, q are selected numbers.

$$\begin{array}{ccc}
 & C_1m^2 - m(n+p) + q & \\
pm - q & & mn - q \\
 & q & \\
C_3p^2 - p(m+n) + q & & C_2n^2 - n(p+m) + q \\
 & np - q &
\end{array}$$

Fig. 5.

Suppose for instance it is required to make numerical patterns so that $r_{xy} = \cdot 4$, $r_{yz} = \cdot 5$, and $r_{zx} = \cdot 6$. First find C_1, C_2, and C_3 and then substitute *any* values for m, n, p, and q taking care however to avoid negative values for the cells. A large number of patterns may thus be made. For example take $m = 6$, $n = 5$, $p = 9$, and $q = 12$, which will give the following pattern (see Fig. 1) $W = 12$, $P = 42$, $O = 18$, $G = 33$, $R = 3$, $Y = 12$, $B = 21$. Now calculate the correlations on the plan of equation (3), and they will be found to give the required values.

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[1] See the distinction drawn in the footnote, page 93.

Further it can readily be shown that Fig. 5 is perfectly general, *i.e.* that every possible pattern can be made by its aid. In practice the patterns in the foregoing paper have been made so as to give accuracy to two places of decimals in the correlation coefficients. It must always be remembered that if the seven cells are multiplied by the same number, the resultant pattern will give the same correlations.

APPENDIX II.

Equations (6), (7) and (8) in the above text may be derived from the Bravais formula for the correlation coefficient.

Let m = a small error committed in a measurement a,
,, n = ,, ,, ,, ,, b,
,, etc. ,, ,, ,, ,, etc.

We have
$$x = Am + Bn + Cp + \text{etc.},$$
$$y = A'm + B'n + C'p + \text{etc.},$$

where x is the resultant error in the first variate, and y is the resultant error in the second variate.

The Bravais coefficient[1] can be written

$$r_{xy} = \frac{AA'\sigma_m^2 + BB'\sigma_n^2 + CC'\sigma_p^2 + \text{etc.}}{\sigma_x\sigma_y} \qquad \text{...........(9).}$$

If the measured elements are dice scores, then

$$\sigma_m = \sigma_n = \sigma_p = \text{etc.}$$

We then have

$$r_{xy} = \frac{AA' + BB' + CC' + \text{etc.}}{\sqrt{(A^2 + B^2 + \ldots)(A'^2 + B'^2 + \ldots)}} \qquad \text{...........(10).}$$

Further, in the case of dice scores which are added together,

$$A = A' = B = B' = \text{etc., except that some are zero.}$$

The measured units a, b, c, etc. are now represented by the individual dice. The question now arises concerning dice whose scores are considered negative. For instance, let the die b be common and its score negative for both x and y. B and B' in (10) are then each negative, but BB' remains positive and r_{xy} is the same as before. Let a be a specific die in x

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[1] I am indebted to Dr G. H. Thomson's manuscripts for equations (9) and (10).

and its score negative; then A is negative, A is zero. AA' is zero, and A'^2 is zero, but A^2 is positive. Again r_{xy} is the same as before. The statements on pages 87, 88, are thus accounted for. It remains to ask how negative correlation can be produced. The denominator in (10) is always positive. A term in the numerator such as BB' can only be negative when B and B' are of opposite sign, that is, the score of this particular die, in order to produce negative correlation, must add its score to one variate and subtract it from the other. The die b and all other such dice then give negative products in the numerator of (10) and become a source of negative correlation. Thus equations (6), (7) and (8) arise, when dice producing negative correlation are present.

(*Manuscript received* 29 *May* 1919.)

ON LISTENING TO SOUNDS OF WEAK INTENSITY.

By E. M. SMITH and F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

PART I.

A. Introduction.
  1 *The origin of the present research.*
  2. *The aim of the present research.*
B. Apparatus and methods.
C. Results.
  1. *Constancy of ranking from day to day.*
  2. *The effect of the daily order of testing on ranking.*
  3. *The effect of the range on the number of errors and false responses.*
  4. *Comparison of methods.*
  5. *The duration of the stimulus as a factor determining the response.*
  6. *The effect of a warning signal.*
  7. *The effect of light and darkness.*
  8. *Distribution of errors and false responses.*
  9. *Practice and improvability.*

A. INTRODUCTION.

1. *The origin of the present research.*

DURING the early months of 1918 it was realised that many of the difficulties presented by the various acoustic devices which were being employed for the detection of enemy submarines involved for their solution the careful selection of operators. Thereupon the Lancashire Anti-submarine Committee, mainly owing to the influence of Mr. A. P. Fleming, O.B.E., secured the establishment of an officially recognised psychological selection base where candidates for the anti-submarine service were examined. Tests were devised with a view to the detection of special abilities in listening. This work raised a number of problems largely of theoretical interest which could not be properly dealt with at the selection base itself. The Staff of the Cambridge Psychological Laboratory, which had been intimately concerned with the work almost from its beginning, therefore arranged to carry out research into certain of these points at Cambridge. One of the tests concerned auditory acuity, and the present paper has grown out of the more detailed investigation of the conditions under which sounds of relatively weak intensity may best be heard.

The work reported in this paper has been carried out under the general direction of Dr C. S. Myers, F.R.S., to whom we owe much for his advice.

2. *The aim of the present research.*

The two problems which were definitely attacked in this connexion were: (i) to devise apparatus and methods by which a satisfactory auditory acuity test may be secured, replacing the commonly used single intermittent stimulus by a continuous sound, and (ii) to observe in detail the influence of various objective and subjective factors upon successful listening to sounds of weak intensity. It was considered necessary to use a continuous source of sound as a stimulus because of the desirability of approximating to the conditions of actual listening for submarines. And it was initially evident that listening, even to very simple sounds, may depend on a variety of complex conditions. We have tried to get further and more accurate knowledge of the nature of these conditions, and of their effects.

B. APPARATUS AND METHODS.

Apparatus.

All our early tests and experiments had been carried out with an ordinary Politzer acoumeter[1]. This instrument proved extremely unsatisfactory owing to the many uncontrolled variable factors involved. Slight changes in the pressure exerted by the operator on the lever of the instrument, or small deviations of the instrument from the perpendicular, are sufficient to cause considerable variability in the intensity of the sound. Any form of apparatus, also, which involves movement, either of the listener in relation to the source of sound, or of the source of sound in relation to the listener, must be unsatisfactory, owing to its dependence upon varying acoustic properties of the rooms in which the test is applied. In its simple form, therefore, the Politzer acoumeter was very speedily discarded, and, as a first modification, what may be called the 'boxed acoumeter' was used.

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[1] This consists essentially in a small metal lever the weighted end of which, falling through a constant distance on to a metallic bar, produces a sound.

A Politzer acoumeter was carefully mounted inside a weighted box, so that it was held in a fixed horizontal position. An opening at the back of the box admitted the experimenter's hand for the purpose of manipulating the lever of the acoumeter. In the front of the box was a second opening over which cardboard shutters, with apertures of varying size, could be placed. This simple device secured the required variation in the intensity of the sound, while it avoided the necessity of any movement, either of the sound, or of the listener; at the same time the disturbing effects due to the acoustic properties of the room in which the test was carried out were considerably reduced. The results were more definite with this form of the instrument, but were still disturbed by uncontrollable variations due to freedom of play of the fall-hammer of the acoumeter. Further, the single intermittent sound was unsatisfactory for our purpose; and we had more than a suspicion that listening in free-air does not yield results directly comparable with those that may be obtained by listening over a telephone circuit, after the manner which was at that time required for the detection of submarines. It is obvious that when a subject is listening to a sound coming to him through free air it is far more difficult to eliminate distracting noises. For this very reason the boxed acoumeter proved of special service in obtaining definite results in some investigations concerning the effect of distracting sounds, which will be described in the second part of this paper.

Two other devices were used before we finally settled down to the investigations with a continuous sound. First an electrically controlled instrument was tried, the sound stimulus being the click produced by the fall of a small steel rod on to a metal base. The source of sound was mounted upon a scale and could be moved to different distances from the observer: in this way the necessary variations in the intensity of the sound were obtained. The sound was conducted to the subject's ear through a tube about three feet long, which was provided with an earpiece so as to cut out external disturbing noise. The necessary movement of the source of sound was reduced by the introduction of an iris diaphragm into the tube at the end farther from the listener, between whom and the source of sound a screen was interposed. The instrument, which may be called the 'tube and screen' audiometer, proved unreliable and, in particular, gave rise to much difficulty owing to the interference of sound waves reflected from the end wall of the testing room[1]. Consequently, listening to sound coming through the free air was entirely discarded, and telephone listening was adopted in its stead.

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[1] In fact all our observations tended to confirm the conclusions reported by Pillsbury as to the unreliability of the drop methods of producing sound; *vide* "Methods for the Determination of the Intensity of Sound," *Psychol. Monogr.*, 1910, xiii. 5–20.

The first kind of sound we employed was a click of varying intensity produced in the telephone receivers, the intensity being controlled by a series of graded resistances. The test devised for use with this instrument gave considerable satisfaction in practice, but was still open to the objection that an isolated click in no way represents the continuous sound of a submarine's engines. Hitherto we had been almost entirely preoccupied with the practical tests and had kept to the use of the click because little was known concerning the reliability of methods which employed a continuous sound. Now, however, we decided to take up definite research on the apparatus and methods needed for an acuity test with a continuous sound stimulus.

Apart from the practical value of the use of a continuous sound stimulus for the present purpose, the latter possesses certain obvious general advantages. Most sounds of daily experience are continuous in nature. The test itself is more interesting and tends to call forth more of the 'psychological' qualities of the listener. With a continuous sound it is easier to keep a definite check upon the observer's readiness to respond. He may be kept listening throughout a certain period during a portion only of which the sound is present, and note may be taken of the exact time elapsing between the actual onset of the sound and the response which marks the subject's reaction to it.

The source of continuous sound which we used in the earlier experiments was provided by an ordinary buzzer, such as may be purchased at any electrical stores. Owing to war conditions, apparatus by which we had hoped to produce a more constant sound was delayed for a long time. The buzzer which we were forced to use varied considerably on different occasions, and the variations were too rapid to admit of control. This renders many of our results difficult to interpret with any strict degree of accuracy. We have now experimented with several different types of buzzer, but have found nothing to give such reliable and steady results as the electrically maintained tuning fork with mercury cup contact, bridged by a condenser, which we employed in the final series of tests. As regards pitch, the original buzzer gave a dominant tone of about 900 vibrations per sec.; the pitch of the tuning fork of which we made most use was of 400 vibrations. Clearly, so large a difference of pitch might be expected to produce significant differences in the results. As a matter of fact, very little direct comparison of the results which we obtained with different settings of the apparatus is possible, but significant differences do not appear to have occurred. The precise effect of variation in pitch upon experiences at the threshold must be left over for more careful investigation.

In the first setting of the apparatus the buzzer and the main resistance were placed directly across the battery, and a galvanometer was introduced into the telephone circuit to give a measure of relative strengths of current used. (See Fig. 1.) With this setting a well-marked click in the telephone receivers was obtained every time the buzzer was switched on or off. In order to allow of 'catch' experiments, a second circuit was arranged giving a click, but no continuous sound.

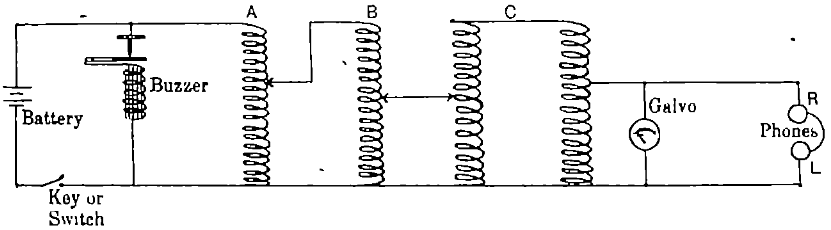

Fig. 1

It was desired, however, to eliminate the click altogether, since it acted as a warning and proved to be audible when the continuous sound could not properly be heard. With our first setting it was obvious that the

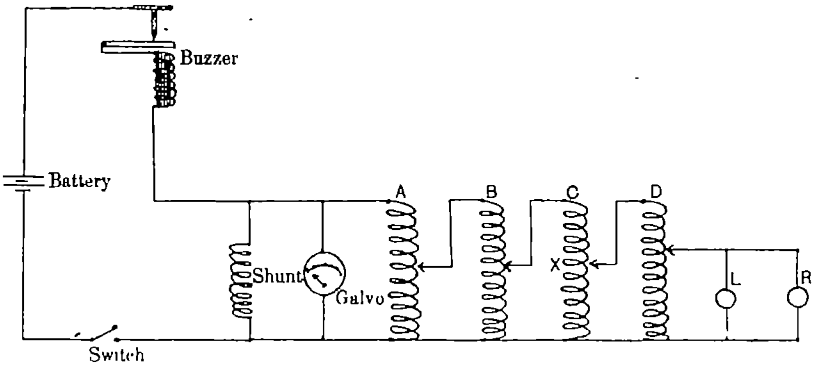

Fig. 2.

telephone receivers were being actuated by direct current from the battery, before the buzzer itself actually started up. This effect was removed when the buzzer was placed in series with the main resistance, and the click now disappeared. It is not clear why the click should have been completely eliminated by this arrangement, but possibly the diaphragms were always slightly in vibration owing to induced current: this, reducing the amount of inertia to be overcome when the buzzer was switched in, may have accounted for the disappearance of the initial and final clicks. We now placed the galvanometer across the first resistance, and used it merely as a check upon the constancy of the total current employed[1]. As a matter of fact the buzzer contacts proved increasingly faulty and variable, and gave a great deal of trouble. With this setting we obtained our measure of the threshold by taking a reading from the scale on one of the resistances, a finely graded potentiometer (see Fig. 2).

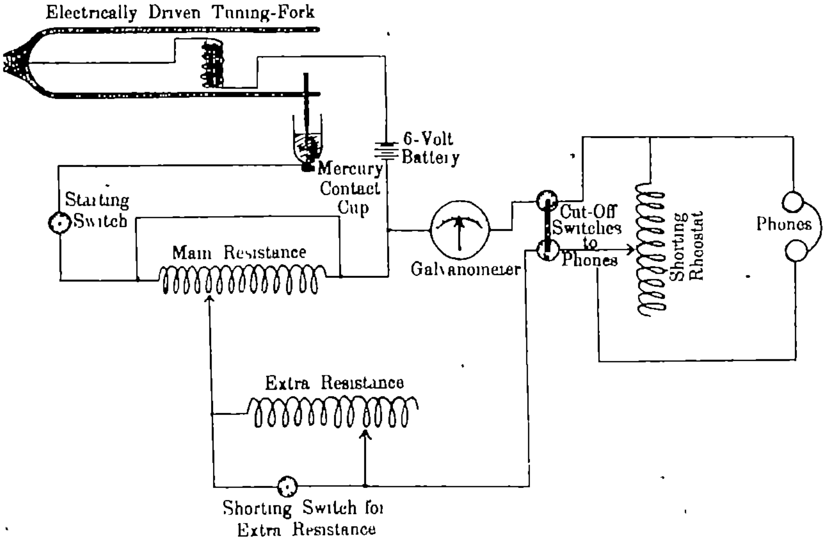

Fig. 3

In the final setting, a tuning fork was placed in series with our main resistances, the galvanometer was replaced in the telephone circuit, and a shorting rheostat used to increase the possible range of intensities. A defect of the earlier setting had been the necessity for starting and stopping the source of sound at every trial. This was obviated by the use of a double cut-off switch to the telephones. Thus the tuning fork was in vibration during the whole period of testing, the sound being switched into, or out of, the telephones by the use of the cut-off switch. The threshold was obtained in terms of the strength of current employed, as indicated by the galvanometer (see Fig. 3). This setting of apparatus is perfectly simple to apply, and appears to yield very reliable results in practice. There was throughout not the slightest trace of a click in the telephone receivers.

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[1] This alteration was made on the suggestion of Mr H. Pealing, who was sent by the Lancashire Anti-Submarine Committee to give advice on the matter of apparatus.

Hereafter the results obtained from the first setting will be referred to as Series I, those obtained from the second setting as Series II and Series III[1], and those from the final setting as Series IV. In Series I, II and III, each ear, and both ears, were separately tested[2]. For the purpose of testing one ear by itself one of the telephone receivers was cut out, by an alteration in the attachment of the leads to their terminals in the observer's room. That is to say the same total current was flowing through the circuit, but was being received at one ear only. The diminished resistance consequent upon the cutting out of one receiver of course meant that the same scale reading on the apparatus represented a somewhat greater intensity of sound. Probably a better arrangement would be to use a dummy telephone, so that when a single ear is being tested that ear may receive approximately the same amount of current, for the same setting of the apparatus, as in binaural testing.

In most of our experiments, the subject's response was effected by the manipulation of a two-way switch in circuit with two differently coloured flags mounted on ordinary time-markers. Movement of the switch in one direction depressed a red flag and indicated that the observer had begun to hear the sound; movement in the other direction similarly actuated a green flag and was a sign that the sound was considered to have stopped. The necessity of noting all this, as well as of making the required adjustments, and of reading the galvanometer, placed a considerable strain on the experimenter. We are, therefore, in course of testing devices designed to record automatically the onset and duration of the stimulus, and the relation thereto of the subject's response.

Obviously, in order that the results obtained by any of the methods just described should be assessed at their proper value, accurate knowledge is required concerning the relation between the amplitude of vibration of the telephone diaphragms which are being used, and the strength of the current employed. We have made several attempts to determine this, and have secured some interesting results which will be described later[3].

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[1] See p. 109 for description of Series III

[2] The left ear of subject B. was decidedly sub-acute and for this reason it was not tested alone after Series I.

[3] See Appendix to second part of the paper.

Throughout the whole series of our investigations, the listener sat in the sound-proof room of the laboratory The sound was conducted from an adjoining room, in which the experimenter sat, and consequently the observer was entirely undisturbed by noises coming to him through the free air, other than those occasioned by his own movements. We were alternatively subject and experimenter in most of our observations, but as often as possible other subjects were used as well.

Methods.

The first method employed was to present sounds which were gradually decreased in intensity while the subject listened. Ten trials were made, the listener responding by the use of one switch when the sound was heard, and by another when it ceased to be heard. The threshold was taken as the mean of the ten disappearance values. Then a corresponding ascending series was taken. Later a method of mixed intensities was employed, and this yielded far more definite results. A continuous sound was kept going for five seconds at a given intensity, the subject responding in the usual manner if he perceived it. Then a different intensity value was substituted, until the whole range of intensities required had been covered. In order to obtain a threshold, each intensity within the range was presented ten times; each ear separately, and both ears together being examined. In all these cases a signal preceded each test, the warning being given by the flashing of a small electric light. The intensities used were mixed irregularly over such a range as to include values very readily appreciated, some that were appreciated only with difficulty, and others below the threshold. 'catch' experiments, in which no sound followed the signal, were interspersed. This method, which will readily be recognised as an application of the method of 'right and wrong cases,' was found to yield the best results. Clearly it is equally suited either to an intermittent or to a continuous source of sound. It is to be preferred to the method of gradual ascent and descent.

The whole of the experiments so far carried out may be grouped in four series. The first series comprises all the cases in which the continuous sounds were preceded by clicks. It is noteworthy that under these conditions even when the continuous sound had passed below the threshold, the click remained readily perceptible.

In the second series we varied the external conditions of listening. This series comprises tests in which no click was present, in which listening in bright illumination was compared with listening in the dark, and in which the warning light signal was sometimes present and sometimes absent.

Comparison of the results obtained in light and darkness suggested that interesting effects might possibly be produced by the use of coloured lights. As, in our observations under these conditions, we made a slight readjustment of the various resistances, the results obtained will be grouped together to make up the third series.

For the fourth series the final setting of the apparatus was used. We were now particularly concerned in the attempt to find evidence for or against auditory fatigue. The series therefore consisted of sittings extending over two hours' continuous listening. For the first group sounds of varying intensity were introduced at intervals, the subject responding each time he thought he could hear a sound; and for the second group the sound was kept on most of the time, and was interrupted at intervals, the subject responding as soon as he failed to hear. No detailed account of the results obtained from this series will be attempted in the present paper. The material secured raised a number of problems which call for more detailed investigation, and it will be more convenient to deal with the whole of these questions together in a later communication.

C. RESULTS.

A study of the results obtained from the whole series of experiments falls naturally into two main divisions. First we shall consider the conclusions that may be drawn from classifications and analyses of the actual responses secured, at the same time making use of explanatory statements of the observers where these seem to be required. Afterwards we shall consider the significance of such of the introspective remarks as have not been adequately reviewed in connexion with the objective study of results.

1. *Constancy of ranking from day to day.*

By ranking is meant the arranging in their relative order of efficiency of right, left, and both ears for a given setting. Thus, although the actual thresholds obtained might vary greatly from day to day[1], the ears might yet present a fairly constant order of ranking over a long period. And from a study of the ranking in Series I (see Table I) this appeared to be actually the case.

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[1] Cf. C. S. Myers, *Reports of the Cambridge Anthropological Expedition to the Torres Straits*, II. Pt. II. p. 145, 1903: "The acuity of hearing, probably more than that of most other senses, varies considerably from hour to hour and from day to day, according to the physiological condition of the observer."

Table I.

Subject	Series	Both Ears			R. Ear			L. Ear		
		Rank 1	Rank 2	Rank 3	Rank 1	Rank 2	Rank 3	Rank 1	Rank 2	Rank 3
B.	I	10	—	—	—	10	—	—	—	10
	II	13	8	—	10	11	—	—	—	—
	III	4	11	—	13	2	—	—	—	—
	Total	27	19	—	23	23	—	—	—	—
S.	I	7	3	—	5	4	1	1	2	7
	II	15	3	2	5	10	5	5	11	4
	III	5	5	5	6	5	4	8	4	3
	Total	27	11	7	16	19	10	14	17	14

The figures in the vertical columns under ‘Rank 1,’ Rank 2,’ ‘Rank 3’ respectively, indicate the number of times in a series the given ear is so ranked.

Extended observations, however, carried out in Series II and III, made it clear that the order of ranking may undergo considerable change from time to time. For the purpose of comparison of right- and left-ear listening, subject B. may be ruled out, as hearing in his left ear is so subacute that by itself it is useless for the present purpose. But S. shows an interesting change, from a period at the outset, in which the right ear tended dominantly and consistently to rank above the left, through a period in which right and left ears appeared practically equal in ranking (in which that is to say their positions in first, second, or third ranks were almost interchangeable) to a period in which the left ear appeared slightly better than the right. As the separate testing of each ear was effected simply by the reversing of the telephone receivers on the head, no question of difference in relative sensitivity of the telephones can arise. It appears quite possible that, except in the case in which large differences exist, relative acuity in uniaural listening may undergo periodic change, and this conclusion tends to be confirmed by a prolonged series of earlier investigations with S. for subject, the details of which are not here reported.

Far more interesting however, is the study of binaural and uniaural differences considered from the point of view of order of ranking. As already mentioned (p. 107) our uniaural testing was effected by cutting out one of the telephone receivers. With this arrangement the right or left ear in the uniaural test must have been getting about double the strength of current which it received in the binaural tests.

On the basis of lengthy and repeated tests carried out on women students in the summer of 1918, when the boxed acoumeter was being used, it became perfectly clear that, where differences existed in the acuity of the two ears, uniaural hearing was on the whole superior to binaural.

This was undoubtedly due in the main to the fact that the attitude adopted for binaural listening in the Politzer acoumeter test, that of facing the sound, is unfavourable, as compared with the case in which one ear is directed definitely towards the source of sound.

With our present method, however, we found binaural listening to be clearly superior. This was particularly noticeable in the case of B. His left ear was so much below normal that with this ear alone he was quite unable to hear sounds at the binaural threshold. Nevertheless the binaural ranking was at first distinctly higher than that for the right ear. These results are entirely in agreement with those of earlier observers, but the superiority of binaural listening demands special and more careful investigation, and will be made the subject of a later paper.

2. *The effect of the daily order of testing on ranking.*

We varied the order in which the different thresholds were taken from day to day. Thus, for B. the uniaural threshold might be taken before the binaural, or the binaural before the uniaural; while, for S., either ear, or both ears, might occupy any one of three positions, and be tested first, second or third. Had it not been for this precaution a comparison of the uniaural and binaural thresholds would clearly have been rendered difficult owing to the effects of fatigue, adaptation and attunement. In computing the results it was necessary to consider each of the three series by itself. The plan adopted was to find the average values of each uniaural, and of the binaural, thresholds for a given series, taking into account position in the order of testing. This gives three average values each for right, left and both ears according as they were tested first, second or third respectively. The sets of uniaural thresholds, and the binaural thresholds, were then each compared among themselves, and all the instances in which the first position in the order of testing coincided with the first rank were grouped together, the second and third positions in regard to the order of trials being similarly treated. As Table II indicates, for B. position 1 in the order of testing coincided with rank 1 in the order of efficiency four times, while position 2 was only twice in the first rank, three times in the second rank, and once in the third rank. A similar relation is even more striking in the case of S., position 1 coinciding with rank 1 six times, while positions 2 and 3 were of rank 1 only twice and once respectively.

Table II.

Subject	Position	Rank 1	Rank 2	Rank 3
B.	1	4	2	—
	2	2	3	1
S.	1	**6**	2	1
	2	2	**6**	1
	3	1		**7**

It therefore appears that the order of testing is by no means without influence, the first threshold to be tested on a given day being to some extent more favourably situated than those which are tested later.

A more detailed summary of the results may be expressed differently. The *actual* thresholds obtained on a given day, instead of the average of a group of thresholds for a given ear, are now ranked in order of delicacy. The number of times in which rank and order coincide is then determined by inspection, and the exact differences occurring in the remaining cases are also ascertained. Thus + 1 and + 2 in the table below indicate that a given threshold ranks respectively one or two places higher than the order in which it was determined on the day in question, while − 1 and − 2 indicate a ranking lower than the order. The results of B., in Series I, are ignored for the purposes of this table, since in this series, on every occasion, the binaural threshold was the lowest, that of the left ear the highest, and that of the right ear intermediate. Consequently the order of testing produced no significant effect; or if any effect was present it was masked by other factors.

Table III.

	Subject B.	Subject S.
No difference	38	45
+1	19	36
−1	15	26
+2	—*	18
−2	—	10

* As only two thresholds (binaural and r. ear) were determined for subject B. each day in Series II and III, the largest difference that can obtain between rank and order of testing in his case is ±1.

This table serves to bring out another fact, that on the whole there is a tendency for a particular threshold to rank higher than the mere order of its testing would warrant. The order of testing, that is, appears to be more influential in improving the rank of a given threshold than in bringing about the opposite result. It may be that listening is characterized by an 'initial spurt,' followed by a long period of 'plateau,' and this conclusion appears on the whole to be borne out by our later results obtained from continuous testing over long periods.

3. *The effect of the range on the number of errors and false responses.*

Errors are defined as failures to respond to the stimulus; false responses as cases in which a response was given when the stimulus was not presented. 'Catches' are here included under 'false responses[1].'

The limitation of the stimuli presented in a given series to those perceptible only with difficulty was not nearly so unfavourable as might have been expected from the observers' remarks concerning their attitude under such conditions. One of us several times complained that when there were very few clearly audible sounds, she felt thwarted and judged her attitude to be highly unfavourable to success in listening. The actual results, however, suggest that on the whole a small range of sounds of weak intensity is favourable to a low threshold. This is well shown by the following examples.

Table IV.

Subject	Range of intensities*	Threshold	Subject	Range of intensities	Threshold
B.	13·5–1·9	4·9	S.	6·5 –?	4·0
Left ear	12·0–2·2	6·0	Right ear	5·0 –1·8	3·8
	12·0–1·9	4·9		3·75–?	2·4
	12·0–1·5	4·2		3·5 –?	2·6
	11·0–2·0	4·5		3·0 –?	2·0
	6·5–1·5	3·2		3·0 –0·5	2·0
	4·2–0·5	3·0		3·0 –0·1	1·0
	4·2–1·0	2·8		2·5 –?	1·7
	4·1–1·2	2·2		2·5 –0·2	1·5
				2·3 –0·4	0·9

* The figures here given represent readings of the galvanometer scale, the approach to zero indicating the diminution of intensity.

It may be argued that the experimenter probably notices when the listener seems to be responding particularly well, and then tends to reduce the range of stimuli by cutting out the louder sounds. Were this the case, the low thresholds obtained with weak intensities could be set down solely to some condition favouring the listening attitude of the subject on a particular day. But the relation of low threshold to small range appeared to be far too constant for this to constitute the sole explanation.

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[1] See footnote to p. 119.

When easily perceptible sounds are included in a series the subject probably tends to be rendered less sensitive. Not only does his attitude become more confident, so that he begins to listen less keenly, but he is prone to set up a kind of standard intensity, and to restrict his responses solely to sounds of which he is sure. Beyond this, it appears highly probable that in some way the inclusion of loud sounds in a series actually rendered the ear less sensitive to the weaker intensities[1]. We may therefore conclude that a limited range of weak stimuli tends to lessen the number of errors.

Turning now to the question of false responses, we find that the conditions of the present trials themselves predispose a subject to make responses at fairly short intervals. Such a tendency might be expected to react upon the threshold, so as to make the latter appear unusually low when a large number of subliminal sounds are introduced. But this result was not observed, and the reason appeared to be that long periods of silence commonly have the effect of rendering a listener relatively inattentive. Not only does he lose interest in his task but he also comes to distrust himself, and to regard doubtful sounds as subjective in origin.

While therefore a range which contains a number of sounds of weak, but audible, intensity is favourable to few errors and a low threshold, if a large number of inaudible sounds are used responses to 'catches' may become less common, and the threshold may be considerably raised.

The number of stimuli used in a given series may be restricted in one of two ways: (*a*) by using a few liminal sounds differing but slightly in intensity; (*b*) by employing a few sounds varying by large amounts. The former plan is the more favourable to the listener. And this is so not only for the reasons just given, but because identification which, as we shall show[1], is an extremely important factor in the listening to sounds, is brought far more into play when finely graded differences are employed.

4. *Comparison of methods.*

Each intensity included in the series presented on a given day was repeated ten times. As a rule the different intensities were given in irregular order, but on most days an order of regular ascent, or descent, or of both, was followed at least once.

It might be expected that the method of mixed intensities would be less favourable for the discrimination of faint sounds than a method of regular descent, and that the latter would be preferable also to the method of regular ascent. In the accompanying table (Table V) are set forth the number of times that each method was employed, together with the percentage number of errors for each series.

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[1] This will be considered in the second part of this paper.

Table V.

Subject	Ear tested	Regular descent method		Regular ascent method		Method of mixed intensities	
		No. of times series given	% errors	No. of times series given	% errors	No. of times series given	% errors
B.	Both	38	2·3	33	2·5	460	2·4
	R.	34	1·7	32	2·1	460	2·3
S.	Both	40	1·4	35	2·4	450	2·1
	R.	47	1·4	45	2·4	450	2·2
	L.	39	1·3	32	2·3	450	2·2

Comparing the errors incident to the mixed method with those incident to the other two methods we find that while the difference is insignificant for B., a far more definite relation appears to hold for S. For S. the mixed method produces a result intermediate between those obtained by the two regular methods. At the same time the superiority of the method of regular descent over that of mixed intensities is more pronounced than is the superiority of the latter over the method of regular ascent.

Both subjects respond to fewer sounds with the method of regular ascent than with that of regular descent, the tendency being unmistakeable in the case of S., but only very slight in that of B.

Thus, whereas B. may be adversely influenced in a slight degree by the presentation of auditory intensities in an order of regular ascent, he is almost wholly indifferent whether a mixed, or a regularly descending, order is used. But this relation is definitely reversed in the case of S., the method of regular descent being for her easily the most favourable order of presentation. S.'s own reports show that she was conscious of this fact. She said that with regularly descending intensities the whole attitude was uncritical and there was some relaxation of attention. Constant strain and definite decision were absent. Identification was rarely resorted to, and the subject was usually well adapted and ready to receive the stimulus. In short, the reactions became much more mechanical. B. reported many times that the order of presentation of the stimuli was indifferent to him, so that in his case also there is correspondence between the reports and the results.

We therefore seemed justified in concluding

(a) that no method of presentation can be regarded as absolutely the best, but that in determining the method to be used individual differences need to be taken into account;

(*b*) that at the same time a method of regular descent as compared with a method of regular ascent probably tends to yield a slightly lower threshold;

(*c*) that the reason for this appears to be the relatively uncritical attitude evoked by the method of regular descent.

5. *The duration of the stimulus as a factor determining the response.*

Our observations have repeatedly shown that up to a time not exceeding five seconds, a distinct correlation holds between the intensity of the stimulus and the reaction-time. Thus while the reaction-time for clearly supraliminal sounds was practically always immediate, that for sounds at the threshold was usually much delayed, seldom being less than three seconds, and often considerably over four seconds. The reaction-time for sounds only just above the threshold was appreciably delayed, but to a lesser degree. It is open to objection that since the duration of the stimulus was the same throughout the greater part of the experiments, and an accompanying warning signal was frequently employed, the subject would fall naturally into the attitude of responding at regular intervals. Consequently, whenever an unusually long interval occurred (as when two or three subliminal sounds were successively presented) the automatic tendency would itself be sufficiently strong to account for the reaction. But the relatively small numbers of false responses, and the almost complete absence of reactions to sounds definitely below the threshold, rob this objection of any weight.

The question then arises how far the prolonged delay of response to weak sounds depended on sensory factors, and how far it was due to difficulties of identification and decision. Certainly the two last-mentioned causes were operative. Whether sensory changes also contributed to the effect is more disputable. But we believe that they did[1].

It is well known that every stimulus requires time to produce its full effect, and within limits it is true to say that the weaker the stimulus the longer is the time required. Kafka, working with auditory stimuli of weak intensity, fixed the time necessary for development at 1·5 seconds[2]. Wells in his study of *Reactions to Auditory Stimuli of Varying Duration*[3], affirms that, with a telephone buzzer as the source of sound, no difference of intensity was perceived when the period of stimulation was increased from 30σ to 106σ and though there was evidence that a difference in intensity was noticeable when the period of stimulation was increased from 7σ to 30σ. Here, then, are two estimates of the period required for the maximal development of sound, each of which falls very far short of four seconds.

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[1] Cf. C. S. Myers, *Text-Book of Experimental Psychology*, I. 30–31.

[2] "Über das Ansteigen der Tonerregung," *Psychol. Stud.* II. 288.

[3] G. R. Wells, *Psychol. Monogr.* 1913, XV. 68 *seq.*

The weaker auditory stimuli which we employed were certainly less intense than that used by Wells, and the same may be true in relation to those of Kafka. These findings, therefore, do not exclude the possibility that the longer delayed responses in our experiments were due to lag in the sensory development of the sound. Urbantschitsch, moreover, as quoted by Luciani[1], allows a longer latent period and states that a "weak tone is only fully appreciable to a sound ear after 1–2 seconds of continuous impression." It is possible that as the lower limits of auditory audibility are approached, the time required for a stimulus to produce its full effect increases very rapidly. The pitch of the stimulus-tone may also affect the latent period. It would be interesting to carry out a series of experiments to determine within what limits the threshold of auditory acuity may be varied according to the duration of the stimulus.

One argument in support of the contention that sensory changes were partially effective in the longer reactions, rests on the fact of the relatively small variability of reaction-time that was shown. If no sound requires more than 1·5 seconds for its development, the further delay being due solely to the factors of identification and hesitation, we should not expect to find the length of the reaction-time increasing regularly with decrease in the intensity of the stimulus. The fact that an inverse relation of this character occurred justifies our belief that sensory changes may be operative to as much as four seconds or more.

Another explanation may be suggested. Even though the period of sensory development of the auditory stimulus is so short as not to exceed two seconds in the longest cases, there may be superimposed upon this a more extended period of attunement. Attunement appears to be a process intermediate in nature between mere sensory response and conscious identification. It marks a state of preparedness, on the part of the subject, to receive a particular stimulus, or class of stimuli. In most of the well-known examples of attunement, such as the motor attunement which occurs during the lifting of weights[2], the subject has repeated experience of a given stimulus. This kind of attunement may occur in our experiments also. We later refer[1] to the fact that a subject is unable at first to pick up sounds which, after a very short time, prove to be well above his threshold. But attunement need not be restricted solely to cases in which the same stimulus is repeatedly experienced. It may occur within the limits of a single experience, provided that experience is sufficiently prolonged. Combining the effects of these two kinds of attunement, we may maintain that it is owing to this factor that the delay of response to weak intensities occurs. The latent period required before a stimulus is reacted to varies inversely with the prior experience of the duration of that stimulus.

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[1] *Text-book of Physiology*, 256–7.

[2] See C. S. Myers, *op. cit.* I. 207–209.

At what point the limits of attunement are reached in the case of a single sensory experience, and when attunement passes over into adaptation and fatigue, do not now concern us. Attunement need not be considered as entirely replacing conscious identification, but rather as a predisposing condition rendering the subject receptive to a sound which might otherwise escape his perception. Particularly in the cases in which the sound is exceptionally faint, an observer may still require to go through a process of conscious identification before he finally makes the response.

6. *The effect of a warning signal.*

Two kinds of warning signal were used, (α) the click which accompanied the making and breaking of the circuit in Series I (see p. 105), (β) a light signal from a small electric lamp which was given coincidently with the auditory stimulus and remained on as long as the latter was continued. In the 'catch' tests the light signal was presented by itself.

(α) *The Click.* This was eliminated from the three later series, so attention must be confined to Series I. A characteristic of this series was the remarkably small number of responses to 'catches.' The clicks used in the 'catch' tests of this series, unlike those associated with the sound stimulus, were approximately of the same intensity throughout, and were louder than the click accompanying the weaker of the continuous sounds. B. soon became conscious of this fact, but S. never appeared to notice the difference. The small number of 'catch' responses, then, may have been due to the fact that the subjects were influenced, either consciously or unconsciously, by the difference in quality and intensity of the 'catch' clicks.

The clicks may also in some cases have helped to lower the threshold. B. reported: "I could not distinguish between the clicks to-day. When the clicks are faint I know the sound is in, and feel a tendency to respond." S. believed that the warning afforded by the click was helpful, since the click itself seemed to be modified in a peculiar manner when succeeded by a sound. In this way she thought she was able to detect indirectly sounds which might otherwise have been below the threshold.

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[1] P. 128; also see p. 114.

Again, the clicks were usually distinctly audible, even when the continuous sound was liminal or sub-liminal. Owing to this fact, the subject came to limit the period of intense concentration to definite intervals[1]. At present the evidence carries a slight indication that the periodic relaxation of attention thus induced was unfavourable to discrimination. This is another point on which a practised differs from an unpractised subject; for the latter distinctly prefers to have the warning click, or some equivalent signal.

The initial click, as a rule, appeared to be somewhat louder than the continuous sound it introduced. It has been shown earlier in this paper, that the introduction of loud sounds into a series, appeared to influence adversely the subjects' ability to detect fainter sounds[2]. In so far as this is the case the click signal does not make for the lowest possible threshold. This signal appears to exert two counter-influences. On the one hand, it provides an additional basis for discrimination and inference; on the other, it may tend to render the subject somewhat less sensitive to the stimulus.

The limiting of fixed attention to certain well-defined intervals seems unquestionably favourable to the reduction of the number of false responses, for with a signal of the present type clearly the number of such responses cannot exceed the number of 'catches' actually inserted by the experimenter. Where no such limit was imposed the false responses were not evenly distributed but tended to fall into groups[3]. This was shown very clearly in the two-hour sittings. The use of the double click signal prevented the appearance of these clusters of perseverative reactions.

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[1] B. reported: "I have an impression that I might not hear without the clicks, but with them no stiffening of attention comes immediately. They are a signal to attend, but unless the sound follows attention is not strained." When the clicks were discontinued, B. said: "The absence of the clicks seems to make it more difficult to attend, because the clicks stir one up."

[2] See p 114.

[3] Definite silent intervals, lasting for five seconds, unless the subject responded before that time was up, were interpolated into the other series in the same proportion as the 'catch' tests were distributed in the series where a signal was used The absence of a signal greatly increased the number of false responses The latter were all recorded, irrespective of whether they coincided with one of the set silent intervals or not. In the case of one of the subjects, B., it was noticeable that a large number of false responses were made almost immediately after the cessation of what appeared to be subliminal stimuli. Cf Knight Dunlap, *Psychol. Rev* XI. 1904, pp. 308–318

In another way the subject was more certain of the sound when warning signals were present. For if he suspected the presence of subjective sensations, he was able the more readily to identify their precise character by noticing their occurrence during the empty intervals. In this way subjective, as compared with objective, sensations often appeared to possess differences of quality, pitch and localisation, which were then used as criteria in the subsequent reactions. The observer thus felt himself to be more in control of the situation when an accompanying limiting signal was used.

(β) *The Light Signal.* In the present tests the light and sound were usually presented to the subject contemporaneously. Sometimes the light was given without the auditory stimulus, and this constituted a 'catch' test.

The effect of a light signal appears to vary according as the signal is anticipatory to, or contemporaneous with, the stimulus.

In the tests with the 'tube and screen[1]' audiometer the warning signal "Now," which had been spoken by the experimenter immediately before the presentation of a sound, was replaced by a flash of light. Complaints had been made that the spoken "now" proved disturbing, and rendered the subject less able to detect the weak stimulus sounds. But in no case, in this set of tests, did the substitution prove satisfactory. Practised subjects did not readily adapt themselves to the change in the nature of the warning signal. and it was objected that the light was troublesome because of:

(i) the necessity for holding the eyes open, many subjects preferring to listen with closed eyes;

(ii) the switching of the attention from one class of stimulus to another;

(iii) the necessary fixation of two different positions, that at which the light appeared, and that from which the sound was expected[2].

The first objection could not be urged in the later tests, which all took place in a dark room, so that the light could be perceived even when the eyes were held lightly closed. Moreover, since the light was kept shining for five seconds unless the subject responded earlier, it was possible for the listener to ignore it more or less, during the greater part of the test, only referring to it in cases of doubt. This plan was adopted by S., who, in consequence, now ceased to be disturbed by the flash of light, but was, on occasion, able to make use of it as a basis of inference concerning the presence of the sound.

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[1] P 103.

[2] In many cases the sound was definitely localised. Most subjects found it an aid to concentrate attention on this position, and expressed great surprise when the sound appeared to come from a different region.

Thus when the light signal was first discontinued S. remarked: "I think I missed a good many sounds. The series seemed long and unpleasant. I missed the light and found I had used it in two ways; (*a*) when I thought there was a sound, but was not sure, I looked to see whether there was a light or not; (*b*) if a particularly long interval elapsed during which I heard nothing, I looked for the light."

Table VI.

Subject	Series	Ear	Average Threshold: No Signal		Average Threshold: Light Signal			Average No. of Errors		Average No. of false responses	
			Threshold* Resistance	No. of Sittings	Threshold Resistance	m.v.	No. of Sittings	No Signal	Light Signal	No Signal	Light Signal
B.	I	B.	B 7	1	B 4·8	1·64	10	27	22·2	1	1·0
		R.	B 8·2	1	B 6·2	1·86	10	39	28·6	2	0·7
		L.	B 11·3	1	B 12·12	0·20	10	18	37·2	3	—
	II	B.	D 2·1	3	D 3·33	1·11	10	11·$\dot{3}$	16·8	1·0$\dot{3}$	2·0
		R.	D 2·3	3	D 3·22	0·82	10	13·$\dot{6}$	16·2	0·$\dot{6}$	1·8
S.	II	B.	D 2·2	3	D 3·01	0·7	9	13·0	12·5	2·$\dot{6}$	1·0
		R.	D 2·5$\dot{3}$	3	D 3·$\dot{7}$	1·08	9	13·$\dot{3}$	19·0	0·$\dot{6}$	0·$\dot{7}$
		L.	D 2·6	3	D 3·8$\dot{1}$	0·91	9	13·0	15·0	2·$\dot{3}$	1·0

* The figures in each case represent the threshold as indicated by the scale reading on the variable rheostat, the approach to zero representing diminishing intensity.

B. confirmed the effect of the light in increasing confidence. For when the light signal was discontinued he said: "At first the general attitude was quite different. There was a great deal more uncertainty. But this quickly passed off and afterwards I noticed very little difference except that it was more necessary to concentrate on the actual listening" (cf. clicks, p. 119). After a further period with the light signal it was again discontinued and B. reported: "Not very different from the series with the light. But the sound seems to come in more explosively, and it seems more important to catch the sound at the start[1]."

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[1] It is perhaps interesting to record that B. several times, and S. once, remarked that the warning light appeared definitely brighter when no sound was present. For example, B. reports: "Light repeatedly seemed more brilliant when there was no sound, though this was not invariably the case." This increased brightness may perhaps have been due to objective variation of the light. But see Ward, *Psychological Principles*, Cambridge, 1918, p. 69.

Only a few tests in which the conditions differed solely in the presence or absence of a light signal are available for purposes of comparison. The results obtained are summarised in Table VI (p. 121).

There is little or no evidence of any pronounced tendency towards opposite results on the different occasions. But, leaving aside B.'s results in Series I which are, as will be seen, very meagre, there is a slight tendency for the errors to increase in number when the light signal is used.

It appears, therefore, that neither the sound signal nor the light signal can be regarded as of much importance to the listener. They may be used to confirm a judgment as to the presence or absence of a sound, but beyond that have little influence. An anticipatory light signal may however, under some conditions, prove detrimental to listening.

7. *The effect of light and darkness.*

It was arranged in the early tests that the subject should sit in an absolutely dark room, as it was assumed that this condition would favour concentration of attention upon the sound Later, however, this supposition was called in question. It was then decided to try the effect of illuminating the experiment room by light from a bright electric lamp[1]. In order that the light should produce its full effect the listener was requested to keep his eyes open throughout the test. Conditions of darkness and light were alternated for a short period, but the tests were not prolonged, since very little by way of modification of results seemed to be observable. No signal was used in this series of tests. The results obtained are given in Table VII.

In Series III the experiment room was much more brightly lighted than for the earlier series. Considering first the results from Series II, we find that when tested in the dark both subjects show a distinct though slight tendency to a lower threshold, to a diminution of errors, and possibly to a slightly increased tendency to false responses. Both subjects preferred listening in a lighted room, though B. was convinced that darkness produced a more favourable condition for hearing faint sounds. Clearly the number of tests is so small that all of these suggestions must be accepted with reserve.

Partly with a view to seeing whether variation of hue as well as of brightness of illumination might have any effect on sensitivity to sound, and partly to secure favourable conditions for observing the effect of variable moods on listening, we carried out a number of trials using differently coloured lights in the subject's room. Interspersed among the colour tests were two trials in which very bright white illumination was used[1], and one in which each subject listened in complete darkness. The whole set of these experiments constitutes Series III[2].

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[1] The floor, walls and ceiling of the room in which the subject sat had a dull black surface, which considerably reduced the effect of the light. In the later series, however, the room was entirely covered with white cartridge paper; hence a much more brilliant effect was produced.

Table VII.

			Average Threshold*				Average No. of errors		Average No of false responses	
			Dark room		Lighted Room					
Subject	Series	Ear	Threshold Resistance	No. of Sittings	Threshold Resistance	No. of Sittings	Dark room	Lighted room	Dark room	Lighted room
B.	II	B.	D 1·7	2	D 2 $\dot{3}$	3	9 0	13 $\dot{6}$	2 0	0·$\dot{3}$
		R.	D 1 9	2	D 1 8$\dot{3}$	3	11·5	11 0	1·0	0·$\dot{6}$
		B.	D 1 5	1	D 1·75	2	10·0	10·5	1·0	1·0
		R.	D 1·7	1	D 1·9	2	8·0	10·0	0·0	1·0
	III	B.	D 2 5	1	D 3 0	1	21·0	22·0	2·0	5·0
		R.	D 3·0	1	D 3·1	1	16·0	21·0	2·0	3 0
S.	II	B.	D 2·5	2	D 2·3$\dot{6}$	3	10·5	14·0	2·5	0·$\dot{6}$
		R.	D 2 35	2	D 2·70	3	13·0	11·$\dot{6}$	3 0	1·0
		L.	D 2 30	2	D 2·50	3	11·0	14·0	2·5	2·0
		B.	D 1·9	1	D 3·0	2	10·0	13·0	2·0	2·0
		R.	D 1·9	1	D 2·4	2	9 0	13·5	1·0	4·0
		L.	D 1·2	1	D 2·6	2	4·0	13 5	6·0	5·0
	III	B.	D 3·0	1	D 2 8	1	19·0	20·0	3·0	0·0
		R.	D 2·6	1	D 2·3	1	16·0	15·0	1·0	2·0
		L.	D 3·0	1	D 2 3	1	17·0	15·0	2 0	1·0

* The threshold is given in terms of the variable rheostat reading, the approach to zero representing a diminishing intensity.

When we consider the results of the two subjects in this series we discover an interesting individual difference. There is some suggestion, in the case of S., that whereas darkness produces a more favourable condition for listening than moderate illumination, the reverse may prove true when a brilliant light is used[3]. Thus S. did unusually well in the bright light and badly in the dark (Table VII). B.'s result does not confirm this. He did exceptionally well in the dark, but his performance in bright light was distinctly poor, and showed a striking increase in the number of false responses.

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[1] The illumination was provided by three ordinary electric lamps, while the white surface of the ceiling, walls and floor heightened the effect.

[2] These tests were unavoidably cut short.

[3] This subject complained very much of the prolonged sittings in the dark, and was greatly irritated by the experience. She felt discontented and suffered from a lack of stimulation. For concentrated mental work S. always prefers very intense sunshine or a brilliantly illuminated room.

As regards listening in coloured light the results are entirely inconclusive. The blue and green seemed possibly favourable to a low threshold, and the yellow and violet unfavourable, with the red intermediate. In the case of the yellow and violet illumination both subjects agreed that the feeling tone accompanying the whole of the test was unpleasant, and the high threshold may perhaps be attributed to this fact. So far as objective results go however, very little of interest is to be gathered from the few trials which we have been able to make on the influence of light, darkness and colour.

8. *Distribution of errors and false responses.*

In this section we shall continue to distinguish as before between errors and false responses (p. 113), though treatment of one, apart from reference to the other, will not always be possible.

We shall consider first the distribution of errors.

(i) In the determination of a given threshold fewer errors occur in the first half of each day's test than in the second half (see Table VIII, p. 126).

(ii) The smaller number of errors in the earlier half of each test is largely due to a short initial period, confined almost entirely to the first presentation of the series of intensities, for which the percentage of errors is remarkably small (see Table IX, p. 126).

(iii) Fewer errors result when a method of gradual descent is employed as contrasted with a method of gradual ascent (pp. 113, 114).

Before discussing the interpretation of these results it will be well to indicate the chief facts concerning the distribution of false responses.

(i) In the determination of a given threshold a definite decrease in the number of false responses is shown in the second half of each day's test (see Table X, p. 127). This decrease is more marked in the case of S. than in that of B. The phenomenon is also more pronounced in Series III, where no signal was used, than in that of the earlier series.

(ii) The tendency to respond when no sound is present appears to be greatest at the beginning of a sitting; but it diminishes after five to ten minutes, rapidly at first, and then more slowly (see Table XI, p. 127).

(iii) There is some indication that the decrease in false responses is of a temporary character only.

(iv) In tests where no signal is used the false responses show no regularity of distribution but tend to occur in groups, the tendency to such responses recurring at intervals during a prolonged sitting.

Comparing these two sets of results it is evident that a rough inverse relation holds between errors and false responses. Thus the number of errors is smallest in the first half of a day's test, the number of false responses greatest: while the reverse is true of the second half. There is no inconsistency in this opposition; but though various suggestions offer themselves it is difficult to determine, with any exactness, the factors at work.

Thus it seems natural to attribute the increase in the number of errors in the later half of a sitting to fatigue. But the fact that the increase in errors is coupled with a decrease in the number of false responses suggests another explanation. Possibly both results are the outcome of increased carefulness and greater determination not to respond except in the case of complete confidence.

Or again, the result in question may be due not so much to the adoption of a definitely cautious attitude, as to the attunement which comes about as the result of prolonged experience of a given set of conditions. In all probability, the complete explanation involves all three factors. It is very doubtful, however, if sensory fatigue really enters in to any appreciable extent: we suspect rather, that such fatigue as occurs is central in origin, for the following reasons: (*a*) that it is generally agreed that fatigue of the auditory end-organ is extremely evanescent, and that recovery is very rapid; (*b*) that it was repeatedly found in our two-hour sittings (see p. 109) that the longer the duration of a liminal sound up to ten minutes, the greater the chance of its being perceived and of the subject's reacting to its outgoing; moreover, the lag of the reaction was very much shorter after the longer sounds than after sounds of briefer duration[1].

The occurrence of groups of false responses at irregular intervals may, perhaps, be regarded as a fatigue effect. It is conceivable for instance, that the strain of prolonged attention in time overcomes the inhibition normally present; and that in this condition the subject does not discriminate between entotic sensations and those produced by the stimulus proper. Or the releasing of the response by a very faint sound may induce an over-confident attitude in the subject, and set up a whole series of responses because the normal control has been temporarily broken down. The prolonged strain may even *induce* subjective sensations. Or in some cases the phenomenon may be motor in character, depending mainly on the kinaesthetic strain set up in the tense arm and fingers. Motor strain in hand or arm cannot be the sole, or even the main, cause however, since it was involved to a much less extent in our last set of experiments, Series IV, which show the phenomenon of clusters of false responses in its best form.

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[1] Attempts have been made to develop a general fatigue test on the basis of auditory experience. The whole trend of our results goes to show that any such test must be extremely unsatisfactory.

Table VIII.

		BOTH EARS		R. EAR		L. EAR		
Subject	Series	1st half	2nd half	1st half	2nd half	1st half	2nd half	No of sittings
B.	I	118	104	123	163	169	203	10 days
	II	132	159	146	141	—	—	21 ,,
	III	157	203	115	150	—	—	15 ,,
	Total	407	466	384	454	169	203*	
S.	I	127	115	118	110	148	165	10 ,,
	II	119	133	149	159	132	137	20 ,,
	III	121	133	122	141	109	114	15 ,,
	Total	367	381	389	410	389	416	

Total number of errors in 1st half of each day's test as compared with the total number in the second half.

* The figures for B.'s left ear represent the results of Series I only.

Table IX.

		BOTH EARS			R. EAR			L. EAR		
Subject	Series	1st 'set'	Average of 'sets' 2–9	m.v.	1st 'set'	Average of 'sets' 2–9	m.v.	1st 'set'	Average of 'sets' 2–9	m.v.
B.	I	16	22·2	3·60	15	28·6	5·12	19	37·2	4·56
	II	22	29·1	3·32	22	28·7	3·1	—	—	—
	III	27	36·0	4·60	19	25·4	2·72	—	—	—
S	I	30	24·2	3·20	21	22·8	2·40	27	31·3	2·90
	II	17	25·2	2·56	24	30·8	4·36	21	26·9	2·90
	III	14	25·4	2·56	17	26·3	3·04	17	22·3	1·36

Total number of errors in the 1st 'set' of each day as compared with the average number of errors for the remaining nine 'sets.'

In tests of this kind it is very difficult to distinguish between the effects of fatigue and the loss of attunement. Thus the groups of false responses may be primarily due to temporary distraction, for example, a change of position, or a deep breath. We have already seen that the number of false responses is greatest at the beginning of the day's test, when fatigue is, presumably, minimal, but it is noteworthy that attunement cannot, at this stage, have begun to exercise any pronounced influence. In this connexion reference may be made to the fact that a lower threshold was secured when the stimuli were presented rapidly and at even intervals. This is readily accounted for by the better attunement induced by such conditions.

Table X.

		BOTH EARS		R. EAR		L EAR		
Subject	Series	1st half	2nd half	1st half	2nd half	1st half	2nd half	No of sittings
B	I	1	9	5	2	2	1	10 days
	II	21	10	13	13	—	—	21 ,,
	III	26	19	40	26	—	—	15 ,,
	Total	48	38	58	41	2	1*	
S.	I	—	1	6	2	4	3	10 ,,
	II	18	12	16	11	23	20	20 ,,
	III	22	10	16	4	17	13	15 ,,
	Total	40	23	38	17	44	36	

Total number of false responses in 1st half of each day's test as compared with the total number in the 2nd half.

* The figures for B 's left ear represent the results of Series I only.

Table XI.

Subject	'Set' 1	'Set' 2	'Set' 3	'Set' 4	'Set' 5	'Set' 6	'Set' 7	'Set' 8	'Set' 9	'Set' 10
B.*	32	28	22	18	8	16	16	18	18	12
S.	28	31	26	15	22	14	12	14	12	24

Total number of false responses made in each 'set' from 1 to 10; the totals for the three series, I–III, are massed together for the purpose of this table.

* In B 's case these totals are diminished by the fact that his left ear was not tested in Series II and III.

The relatively large number of false responses occurring in the earlier part of a day's test, together with the remarkably small number of errors on the first presentation of the range of stimuli, rather point to an attitude of eagerness at the beginning of the sitting. Frequently some excitement is present, the listener is anxious to do well and has not had time to become discouraged. His attitude is apt, also, to be less critical at the start and his responses more impulsive. As a rule this state passed over into a more stable or persistent attitude, such as indifference, passivity, a calm and critical mood, or even discouragement and vexation. Certainly the fresh impulsiveness often present at the start soon suffered a check of some kind.

The very small number of errors in the first 'set[1]' of each day's tests as compared with the average of the remaining nine ‘sets’ is all the more striking in view of the fact that although the method of procedure adopted in S.’s case[1] favoured a small number of errors (p. 115), the difference is just as marked in B.’s case where no such explanation can be sought.

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[1] The various stimulus-intensities selected for use on a given day were each presented ten times. There were thus ten 'sets' (*i.e.* repetitions of the selected stimuli) for each day's threshold. The order of testing followed was irregular (cf. p. 108).

9. *Practice and improvability.*

Improvability in auditory acuity is almost wholly attributable to the subject’s increasing familiarity with the conditions, and to his adoption of a more consistent method of response; it is not to be regarded as primarily sensory in character. Improvement may be manifested in two ways, either by a lowered threshold or by a more sharply defined threshold and by the diminution in the number of false responses.

It was repeatedly found, in the various forms of acuity tests, that unpractised subjects failed to respond to sounds that proved later to be well above their threshold of hearing. This initial disability to detect faint sounds was least marked in those tests in which the sound was electrically produced, the subject listening through telephone receivers. Undoubtedly the ability to recognise a sound plays a most important part in determining the threshold. This fact will be brought out in much greater detail in a section in the second part of this paper dealing with the evidence from the introspective reports.

The second kind of improvement mentioned also depended largely on increased familiarity with the sound and with the conditions of the test. It consisted in a more sharply demarcated threshold and in a drop in the number of responses given when no sound was present. Improvement of this kind was observed in a few cases only. Some of the chief contributing causes were—the assumption of a more comfortable position by the subject; increased power of concentration, due in part to the elimination of unfamiliarity from the situation; and the adoption of a more consistent criterion in border-line responses. Improvement of this kind invariably manifested itself early in the tests, if at all. Our results, already quoted, show that a sudden drop in the number of false responses often occurs early in a sitting, and this appears to constitute one of the most consistent and best defined modes of improvement.

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[1] In S.’s case the method of gradual descent, which, as we have already seen, is conducive to very few errors, and a low threshold, was employed a much larger number of times for the first ‘set’ of each day’s tests than for any subsequent ‘set.’ The exact figures are: S. method of gradual descent employed 71 times out of 135 for the first ‘set’ as contrasted with 55 times out of 1215 for the remaining nine of each day’s ‘sets.’ Neither this method nor the method of gradual ascent was ever used in any first ‘set’ in B.’s case, yet the superiority of the first ‘set’ as judged by the small percentage of errors, is no less marked. In B.’s case the method of gradual descent was employed 72 times in 960 ‘se

We had hoped to obtain some data as to the nature of the improvement, if any, which follows from long practice. The fact that several different settings of apparatus were used, makes it difficult to deduce any reliable conclusions on this point. By comparing the average thresholds of the two subjects for the various series however, it is possible to get a rough relative measure of improvement. When this procedure is adopted, there seems to be definite evidence of the occurrence of fairly well-marked improvement in the case of S. as compared with that of B. The subjects' introspections also confirm these findings. We give the actual thresholds for comparison in Table XII.

Table XII.

		BOTH EARS		R. EAR		L. EAR	
Subject	Series	Average threshold	m.v.	Average threshold	m.v.	Average threshold	m.v.
B.	I	4·80	1·64	6·20	1·86	12·12	0·20
	II	2·62	0·83	2·62	0·70	—	—
	III	3·82	1·50	2·89	0·77	—	—
S.	I	8·08	1·0	8·20	1·40	9·55	0·90
	II	2·68	0·66	3·05	0·82	2·98	0·84
	III	2·63	0·57	2·78	0·39	2·44	0·39

It is evident from these results, that whereas B.'s acuity was unmistakeably greater than S.'s in the first of these series, S. approaches much more nearly to B. in this respect in the two later series. It is open to objection that this approach to equality is due not primarily to any improvement on S.'s part, but rather to a falling off on B.'s. There is reason to believe however, that such deterioration is seldom so prolonged as must have been the case if it were the sole cause at work.

It may be concluded then, that some degree of practice is necessary before the lowest threshold of acuity can be secured, but it still remains doubtful if prolonged practice produces any further effect of this kind.

(*Manuscript received* 5 *August* 1919.)

PUBLICATIONS RECENTLY RECEIVED.

Mind and Medicine By Dr W. H R. Rivers. Manchester: University Press. Pp. 23. 1*s.* net.

This lecture, like the author's *Dreams and Primitive Culture*, was delivered in the John Rylands Library and is here reprinted from the Library *Bulletin*. It is full of interest to the psychologist and sociologist as well as to the physician whom it may help to educate in the modern methods of truly psychological medicine. After showing how the struggle between the material and the mental schools of healing dates from the earliest stages of society, Dr Rivers proceeds to enunciate the important principle, and the practical consequences, of 'psychical determinism,'—the principle that "every mental symptom has its mental antecedent."

The Experimental Psychology of Beauty. By Prof. C W. Valentine. Revised Edition Edinburgh: Nelson and Sons. 1919. Pp. 128. 1*s.* 3*d.* net

The second edition of this unique little work contains two new chapters dealing with music and rhythm. It gives an admirable resumé of experimental work in aesthetics, particularly that published by British and American investigators.

Shell Shock and Its Lessons. By Profs. G. Elliot Smith and T. H. Pear. Second Edition. Manchester: University Press. 1919. Pp xv + 135. 1*s* 6*d.* net.

This admirable little book, now in cheap form, the second edition of which was first issued in 1917, and reprinted in 1918, has clearly proved its worth. No better introduction to the subject has been written for the general public.

Studies in the History of Ideas. Edited by the Department of Philosophy of Columbia University. Vol. I. New York Columbia University Press. 1918 Pp. 272.

This volume contains the following essays: *Appearance and Reality in Greek Philosophy,* by M. T. McClure; *The Meaning of* ΦΥΣΙΣ *in Early Greek Philosophy,* by Walter Veazie; *An Impression of Greek Political Philosophy,* by Wendell T. Bush; *Francis Bacon and the History of Philosophy,* by John J. Goss; *The Motivation of Hobbes's Political Philosophy,* by John Dewey; *An Attempt of Hobbes to Base Ethics on Psychology,* by Herbert G. Lord; *The Psychology of Ideas in Hobbes,* by Albert G. A. Baly; *Truth and Error in Descartes,* by Robert B Owen; *Spinoza's Pantheistic Argument,* by William F. Cooley; *Berkeley's Realism,* by Frederick J. E. Woodbridge; *A Note on Dr Thomas Browne's Contribution to Esthetics,* by Adam Leroy Jones; *The Antinomy and its Implications for Logical Theory,* by W. P. Montague; *Old Problems with New Faces in Recent Logic,* by H. T. Costello.

As the titles indicate the contributions are of philosophical rather than of psychological interest.

Mathematical Psychology of War By L. F. Richardson. Oxford: William Hunt. 1919. Pp. 50. 5*s.* net.

This is an interesting attempt to express in mathematical language the behaviour of peoples at war. The principal factors here subjected to equations are the "vigour-to-war," warlike activity, vengeance, conquests, casualties and the destruction of wealth, business advantages, war as a source of income, fear, pain, fatigue, and the prospects of success.

Manual of Directions for Administering the Otis Group Intelligence Scale. With an introduction by Dr L. M. TERMAN. Yonkers, New York: World Book Co. 1919. Pp. 37. 25 cents.

This Point Scale was devised by Dr A. S. Otis in 1915 for the measurement of the mental ability of groups of individuals simultaneously and was applied by him during that year to thousands of Californian school-children. The publishers claim that "it does not require the services of a trained psychologist either to apply the scale or to obtain dependable results." The material supplied consists of (i) an examination booklet, in two forms, each containing ten tests, suitable for children of the "fourth grade" and upwards, and adults, (ii) individual record cards, for the tabulation of all relevant data obtained, (iii) log slips for entering the data of each test, (iv) the examiner's Manual, giving general information and instruction in regard to the tests, and (v) the examiner's Key, for correcting the answers by the use of stencils. The tests are of the usual character, *e.g.* 'opposites,' 'disarranged sentences,' 'analogies,' 'narrative completion,' and the relative standing of individuals is given by the 'intelligence quotient,' 'percentile rank' or by the 'coefficient of brightness.' The most successful are classified as "'near' genius or genius"!

La Psychologie du témoignage. Par J. VARENDONCK. Gand: Maison Ad. Hoste. 1914 Pp. 196

The author is lecturer in 'the international faculty of pedology' at Brussels. He gives an account of the work of Stern, Lipman, Binet, Claparède, Jung and others on the psychology of evidence, and a bibliography of nearly two hundred papers.

Psychologie générale tirée de l'étude du rêve Par ALBERT KAPLOUN. Lausanne: Payot et Cie. 1919. Pp. 205. 4 fr 50 c.

The writer makes free use of the following terms, *moi central*, *pointe*, *fonction explicatrice* (referred to as F.E.), *moi automatique*, to construct a fantastic 'psychology' devoid of foundation on actual experience.

The Dream that Comes True. By J. NAPIER MILNE. London: The Epworth Press. 1919. Pp. 192. 5*s*. net.

Occupational Therapy Applied to Restoration of Movement. By Major B. T. BALDWIN. U.S.A. Occupation Therapy Department. 1919. Pp 67.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

General Meetings.

May 31, 1919.	On Listening to Sounds of Minimal Intensity, by F. E. Bartlett and E. M. Smith.
	The Relations of Aesthetics to Psychology, by E. Bullough.
July 12, 1919[1].	Instinct and the Unconscious, by W. H. R. Rivers, C. G. Jung, C. S. Myers, J. Drever, Graham Wallas and W. McDougall.

Sectional Meetings.

(a) *Educational Section.*

April 11, 1919.	Psychology and Education, by T. P. Nunn.
May 7, 1919.	Mental Tests for Vocational Guidance, by C. Spearman.
June 18, 1919.	The Mental Factors involved in Memory Drawing, by P. B. Ballard.

(b) *Industrial Section.*

April 25, 1919.	Psychology and Industry, by C. S. Myers.
July 17, 1919.	Some Impressions of Industrial Psychology in America, by B. Muscio.

(c) *Medical Section.*

May 15, 1919.	Psychology and Medicine, by W. H. R. Rivers.
June 11, 1919.	The Generation and Control of Emotion, by A. E. Carver.

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[1] In conjunction with the Aristotelian Society and the *Mind* Association.

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ON LISTENING TO SOUNDS OF WEAK INTENSITY

BY E. M. SMITH AND F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

PART II.[1]

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D. VARIATIONS IN THE SOUND AS EXPERIENCED.

1. *The Classification of Sounds.*

IN the early stages of the experiment both subjects frequently endeavoured to classify the sounds heard. The value of this classification as an aid to listening will be considered later (see p. 139).

S. began by distinguishing two classes of sounds: (i) the aggressive and (ii) the diaphanous. Aggressive sounds "have a kind of weight," and are "in the ear." Diaphanous sounds are characterized as "far away." "When an aggressive sound comes, you react very quickly so as to brush it away."

Such attribution of weight to sound recalls a similar characterization in the case of colour. Just as the darker colours appear heavier than the lighter, so to S. an aggressive sound seemed to possess weight as compared with other sounds. Bullough suggests that the apparent weight of colours may be due to increased pigmentation[1], and the weighted sound may be another direct illustration of the same principle; that is, an aggressive sound may appear to possess 'more of' a certain quality than the diaphanous sound. But neither in the case of colour nor in that of sound is this an adequate explanation of apparent weight. The specific quality may be pigmentation in the case of colour, and it may be a blend of intensity and mode of production in the case of sound. But the real problem in either case is as to why particular qualities become connected with apparent weight.

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[1] The first part of this paper appeared in this *Journal* x, 1919, 101–129

It is also unsatisfactory to dismiss the association of weight with a perceived 'moreness' of a certain quality as due to "unreflective and immediate inference." This is what Bullough is inclined to do. But everyone must have had experience of the extreme readiness with which the adult observer, working under experimental conditions, tends to speak of "unreflective and immediate inference." Many such cases are merely instances of unanalysed modes of response which are both complex in themselves, and the result of a long process of development. Extremely careful inspection is needed to render their complexity apparent, but an observer who is both practised and cautious will find himself seldom forced to admit an inexplicable simplicity in his experience.

The attribution of weight to colour is probably inferential only in the sense in which any compound sensory experience whatsoever is inferential. There are many conditions under which experience belonging to one sensory mode may give rise to or be accompanied by experience belonging to another sensory mode. The most frequently discussed illustration of this is the synaesthesia which occurs in many persons between sound and colour. But there is no good reason for holding that this is the only type of synaesthesia[2]. Thus it is possible that the attribution of weight to sound may be another illustration of the same type of experience. If this is the case the problem falls into line with the general question concerning the precise relation between sensations belonging to different modes.

At the same time a further possibility is open. Sound and colour are known to be closely related in experience, and so also are weight and colour. It may be that it is owing to the close sensory connexion which holds between sound and colour that an attribute far more commonly applied to the latter came to be assigned by S. to the former. The question awaits further investigation.

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[1] This *Journal*, II. 151.

[2] Cf. *e.g.* 145.

In S.'s early tentative classification which was speedily elaborated, varieties in the sound were related to characteristic modes of the response. This gave rise to the following scheme:

The Sound.	The Response.
(i) Aggressive;	(1) Automatic.
(ii) Certainly present, yet	(2) Has to be decided upon.
(iii) Definitely heard as a sound, appearing objective, but not recognized as that of the buzzer (cf. p. 138).	(3) Varied, sometimes of class (1), and sometimes of class (2).
(iv) Something of which I am entirely uncertain: "There is merely something doing and oscillating" (cf. pp. 143 *seq.*).	(4) Generally not made.
(v) Positive silence (cf. pp. 145 *seq.*).	(5) Not made, but definite relaxation of strain.
(vi) Absent, and yet no sensation of positive silence.	(6) Not made.

In the next classification attempted, attention was concentrated on the characteristics of the sounds themselves, not on the nature of the response. The sounds were then classified as:

(i) Thrusting, (ii) definite but not thrusting, possessing colour, weight, body or volume, and warmth; (iii) very diaphanous, recognized as a sound, but *not heard*, and possessing none of the qualities of (ii), or "if they are present they are not marked"; (iv) cases in which there is complete uncertainty as to whether the sound is present, but "something" is present.

Two points are particularly noteworthy in reference to this classification. The first is the reappearance of an apparent weight of sounds. Here weight definitely appears together with colour, while it is at the same time distinguished from volume. So far as could be ascertained, the weight characteristic was more closely connected with the experience of the onset of a sound than with anything else, and it appeared to be very intimately related to a form of tactile experience.

The second point is that for S. differences of quality (or timbre) were particularly striking. The observer commented on this herself. "A lot of the differences I have described," she said, "are probably differences of timbre. The sound becomes decidedly more complex as it becomes louder, and particularly it gains new low components. The most important thing is the *force* of the sound, by which I mean its initial intensity." And again: "I can't say how far the aggressiveness of sounds is due to (i) intensity or (ii) timbre. If one particular component of the sound is present I get aggressiveness, no matter how faint the sound may be."

Very much later in the course of the investigation, S. gave a further description:

"A loud sound has two parts: (i) a nucleus, and (ii) a penumbra. The nucleus takes longer to develop than the penumbra, the latter being present all the time. The two seem to have nothing in common except the fact that they are both sounds. With faint sounds the nucleus is absent. To this is due any hesitation and wondering whether to react or not. The penumbra is present and is perceived, but it only prepares you for the nucleus, which does not emerge."

It is clear then that in the case of a complex sound, such as the tone of a buzzer, a variation in intensity involves many other variations also. To S. these are distinctively variations of quality, but they appear to be intimately related to forms of experience other than auditory.

B.'s first classification and analysis of complex sounds of decreasing loudness were as follows:

(i) Some sounds are easily perceptible, but are complex. There is a low part, like a kind of hum, and a high part, like a kettle beginning to boil. The higher part is slightly more discontinuous than the lower part. These sounds are localised as a little above and behind the ear.

(ii) A second class is still definitely perceptible as sounds, but is more discontinuous; or rather it is a wave effect more than definite discontinuity. The sound never disappears completely, but it comes and goes. The low component becomes less marked. At this stage, the sound seems to move somewhat upwards; a result which may be associated with the loss of the low element[1].

(iii) Then the sound becomes yet more definitely discontinuous and does not develop until some time after the signal has been given.

(iv) At times the low component now disappears entirely[2]. The high part remains, but loses the characteristic of being localised.

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[1] It is interesting to note that Rayleigh maintained that high pitches produce a relatively discontinuous or fluctuating effect. B. was inclined to attribute the apparent discontinuity in this experiment to the weak intensity of the sound, but the fact that high components in loud sounds appeared relatively discontinuous may suggest that pitch has something to do with the matter.

[2] Hancock, in "The Effect of the Intensity of Sound upon the Pitch of Low Tones" (*Psychol. Monog.* XVI. 161–8), reports that with a tuning-fork of 128 v d. as source of sound, the louder sound is always judged lower than a fainter sound of the same pitch. He found the effect much less marked as the sound was raised in pitch. The tone of the buzzer

(v) Next there is no sound at all, but only tactile sensations in the ear, felt but not heard. (This stage appeared for the left ear only, which, in B., is distinctly of subnormal acuity.)

(vi) Finally the rattling effect of the tactile sensations is lost, but there remains something indescribable.

B. reported that in passing from stage (i) to stage (ii) there was a sensible diminution of intensity, but that in the subsequent stages it became very hard to say whether any further changes in intensity were taking place.

Very little was added to this early analysis. On several subsequent occasions B. remarked that faint sounds appeared higher in pitch. He felt that some relation subsisted between this and his tendency to localise faint sounds high up, but he could not satisfy himself as to the nature of the relation.

When B. turned his attention to the actual attitude adopted in listening, however, his suggested classification had several points in common with the attempts made by S.

(1) "Some sounds," he said, "come in with a burst; there is something explosive from the outset. The attitude is 'here it is!'" These, no doubt, are the sounds which S. called aggressive.

(2) "Some sounds appear merely to slide in. The attitude is 'here it comes!'"

(3) "Some sounds are merely 'there,' in a ghostly sort of manner, and you don't know how they got there. They seem to melt in. The attitude is a slightly hesitating: 'Yes, here it is.'" These appear to coincide with S.'s diaphanous class.

(4) In some cases there is absolute uncertainty: "If the sound is there at all, it seems to be there all the time. The attitude is 'Well it *may* be here.'"

Thus both observers agreed in attempting to classify the sounds presented. In each case the classification was usually effected by reference to characteristics other than that of differences of intensity. To B. differences of quality by themselves appeared perhaps less striking than to S., while specific differences in pitch and discontinuity received from him greater emphasis. Differences in the mode of response and in the listening attitude occurred in both subjects.

The significance of this particular selection of characteristics as a

used in the present experiments is not really comparable to that of a tuning-fork, but it is interesting to find that both observers remark that the low component is most marked when the whole complex of sound is loud.

basis for classification will become clearer when a further point is considered.

2. *The Identification of Sounds.*

There can be no doubt that whether a response is, or is not, made to a presented sound largely depends upon whether the sound can be recognized. Auditory acuity, at least as determined by our experiments, is by no means a function merely of successful sensory adaptation, but of ability to identify what is presented. Both S. and B. noticed this fact again and again in the course of the investigation. "In this series," remarked S. on one occasion, "the sound changed in quality, and I missed some on that account"; and again: "Several times I was convinced I heard a sound, and yet it was so different from the real sound that though I followed its coming in and going out, I did not respond." Later on she became more definite still as to the importance of identification: "I am certain," she said, "that my response depends very much upon familiarity with the sound, and this is particularly the case with the fainter intensities. I am convinced that with the weaker tones to hear is to respond to some sort of impression which is remembered, and not to the mere sound stimulus itself." B. was in entire agreement, and reported quite independently: "The sound was very irregular, and different from what it usually is. A good deal of the difficulty of the response was a difficulty of *recognizing* the sound"; and again: "Much of the difficulty of dealing with faint sounds is due to a difficulty of identification....In many of the instances in which I did not respond at all, I think I did hear a sound which, nevertheless, I was not able to identify."

It is interesting that both observers should agree that identification tends particularly to be called upon in the case of faint sounds. This, indeed, is precisely the conclusion already suggested by a study of the actual results secured in relation to the fineness of grading and the range of the intensities used[1].

Now it is fairly clear that for purposes of recognition, mere differences in intensity must be entirely inadequate. Intensity itself, save perhaps at the extremes, is but a faulty individualising mark, so that to attempt to identify a sound by intensity alone would be something like trying to identify a man by the length of the hair on his head. What is required are the most clearly marked specific characters of the sound to which response has to be made. It is no wonder, then, that a classification of sounds proceeds largely upon a basis of differences of apparent quality, of correlated differences in the impulse to respond, of the varied manner in which sounds appear to break in upon the silence, and of divergences in apparent localisation.

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[1] Cf. Part I of this paper, p. 114.

The marks just enumerated are the very characters by means of which a sound may be individualised most definitely, and discriminated from other sounds. There seems therefore to be no doubt that the tendency to classify sounds (which was particularly active in the early parts of the experiment) was determined by the necessity for identification. It is especially noteworthy that, while the classification was carried out from the very beginning, the value of the recognition of sounds was not commented upon till much later. That is, individual characteristics were singled out and made use of long before their peculiar function in hearing had become clear to the listener. The process forms a very clear-cut illustration of how in experience practical analysis often precedes intellectual, and factors may even come to be used in an almost mechanical way, before the actual value of their use is made apparent. This is one of the processes according to which reactions come to be spoken of as 'natural' and 'inexplicable' when actually they have a perfectly definite history and explanation.

3. *The Localisation of Sounds.*

The preceding remarks on the classification of sounds have already indicated that localisation occurred in many instances. Three different questions must be considered:

(i) What were the determining factors in binaural localisation under the conditions of this experiment?

(ii) Were all sounds given like treatment in this respect?

(iii) Apart from lateral displacement from one ear to the other, were sounds accorded distance and direction of movement, and if so why?

In considering the evidence collected with respect to these three points it has to be remembered:

(*a*) That the sensitiveness of the telephone diaphragms used was not quite the same, that of the right-hand one being very slightly superior;

(*b*) That there were well-marked differences between B.'s two ears, the left ear being very subacute;

(*c*) That S. appeared to be subject to prolonged reversals of the ratio of acuity of the two ears, so that the right ear was the more acute in series I, the left ear slightly better in series II, and the left ear very much better in series III[1].

A careful analysis of the reports shows unmistakeably that while localisation is extremely common, there is very little constancy of reference of sound to a given position between the two ears or in either ear. Selections from some of the remarks made by the observers will demonstrate this.

Taking S. first, we find that on various occasions the following statements were made:

"With the clicks (see Part I, 105) I heard sound between right ear and middle of head. With continuous sound heard definitely in left ear."

"Heard continuous sound in right."

"First heard sound at back of head, then loud continuous sounds on left. Clicks remained localised at back of head."

"Sound in left ear generally, but continuous sound far less definitely localised than click; former tended to move over head from ear to ear."

"All sounds on left."

"All loud sounds on left; weaker ones inside head."

"Sound first on left; then passed to various positions in centre of head. Loud sounds nearer centre than faint sounds, which were in or outside left ear."

"Adjusted left-hand receiver at the beginning. This made sound appear to pass over to left. Then localised it above head."

The variability of localisation which was thus evidenced in the case of S. was equally marked with B., if allowance is made for the fact that the acuity of his left ear is far below normal.

"Faint clicks localised at back of head. Louder clicks in ear or towards front of head."

"Clicks are more definitely localised than continuous sound. Localisation appeared to depend on quality of click. Sometimes sound appears at back of head, sometimes just in front of ear and sometimes in ear."

"The variety in the localisation of the clicks depends on the quality of the sound."

"Clicks localised at back of ear; such clicks have a hollow sound."

"About middle of series suddenly realised I was localising sound at back of head; did so repeatedly."

"Faint sounds appeared far back near the centre of the head, loud sounds in the right ear."

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[1] Cf. Part I of this paper, 110.

From a study of these reports it appears that in answer to the first of the questions above formulated we may suggest:

(*a*) That the tendency to localisation which arises under the conditions of this experiment is not determined by any simple and constant factors, but by a complex of conditions which produce different results on different occasions[1].

(*b*) That one of the most important of these conditions is differences in sound quality, although it may possibly be the case that differences of quality operate only in producing a general tendency to localise, and not in fixing a definite position[2].

(*c*) That there is little evidence of lateral displacement as a consequence of such relatively small differences of intensity as were secured by the variations in the sensitiveness of the telephone receivers used.

(*d*) That the localisation ascribed to a sound may be very definitely influenced by contrast when some other sound has already been localised. This at least is proved to occur when one sound is continuous and the other discontinuous, or when one is loud and the other faint.

The answer to the second question is very definitely in the negative. It has been maintained that the adult leaves no auditory impression unlocalised[3], and that every sound impression carries some 'place idea' with it, though the latter may be extremely vague. Our observations suggest that this is far too general a statement. Conclusions arrived at in auditory experiments have always to be considered definitely in relation to the particular character and mode of presentation of the sound used. The sound stimulus employed by McGamble was a telephone click, and our observations have shown distinctly that in telephone-listening discontinuous sounds appear to be much more readily and definitely localised than continuous sounds. Indeed, in the latter, localisation appeared often to be entirely absent.

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[1] Starch's introspections concerning "The Perception of the Distance of Sounds" (*Psychol. Rev.* XVI. 1909, 427–31) tend to confirm the complexity of the determination of localisation. Differences of intensity, of pitch, and of quality, for different distances, were all operative, and, particularly when one sitting was compared with another, did not always appear to operate in the same manner.

[2] Pierce (*Studies in Auditory and Visual Space Perception*, 1901) made tests with organ pipes and tuning-forks which suggest that judgments of distance are affected by tonal complexity, the richer tones being judged nearer than those more nearly pure.

[3] See E. G. McGamble, "The Perception of Sound Distance as a Conscious Process," *Psychol. Rev.* IX. 1902, 7–13, 353–73; and cf. Geissler, "Sound Localization under Determined Expectation," *Am. Journ. of Psychol.* XXVI. 1915. Geissler found that only three of his observers attributed a spatial characteristic more or less explicitly to sound. For the others sound localisation was an indirect function depending on the spatial characteristics of secondary criteria, such as associated visual or kinaesthetic processes.

But besides being given a position in, or between, the two ears, sounds were often referred to a distance from the observer, and were frequently said to possess directed movement.

Some of the reports made may be quoted in illustration:

S.: "To-day I interpret any variation in the character of the sound chiefly as a distance mark. The sound is nearer or farther, not louder or fainter[1]. Definite sounds seem to be directed straight at the ears; while the faint sounds appear to be moving at right angles to the ears."

"All sounds have direction, movement, and speed. The definite sounds move more rapidly, the faint sounds less so, both coming towards the ear. There is a further class of sounds moving at right angles from front to back. Sensations which are not sounds (cf. pp. 143 *seq.*) appear as small particles moving up and down in a streaming manner."

"The distance marks of sounds are very definite. If the sounds are muffled in quality, they may possess double distance marks. They seem submerged as well as at a distance in a horizontal direction. As they become more intense they rise to the surface."

"The upward and downward distance marks are most pronounced in a regular ascending series. As the sounds become louder, they gain in buoyancy. When the sounds are given irregularly, the inwards and outwards marks are more prominent. When the sound is excessively loud it is regarded as inwards."

B. reported experiences of the same kind: "The faint sounds," he said, "are higher up. The strong sounds are lower down but I can't determine the real basis of this judgment." And again: "A faint sound is farther away and higher up than a loud sound[2]."

It thus appears that the third of the questions formulated at the beginning of this section receives an affirmative answer. But whether a distance mark is to be regarded as an original characteristic of certain sounds, or whether it should be treated as a secondary characteristic applied to sounds by virtue of their possession of other qualities or attributes, is a question hard to decide. S. had some tendency to experience sounds as active, and it is not difficult to see how a distance mark might grow out of an attributed movement. Sometimes also she was inclined to endow a sound with some sort of visual form, but in this case it appears more probable that the form was dependent on the distance mark rather than that the distance mark was dependent on the form. What seems quite clear is that the distance mark was not solely determined by differences of intensity. The tendency present in B. to assign a higher position to a weaker sound may have been due to the fact that a weaker sound almost always appeared slightly higher in pitch. But this explanation by association did not appear to the subject to be entirely satisfactory.

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[1] Contrast E. G. McGamble's results: She gave pairs of stimuli which either were given at the same distance from the subject but were of different intensity, or else were of the same intensity but were placed at different distances from the subject. McGamble reports: "Judgments of 'nearer' and louder,' of 'farther' and 'softer' proved to be particularly interchangeable. The subjects showed a marked tendency, however, to say more often that a sound was louder when it was louder only in virtue of being nearer, than to say that it was nearer when it was merely louder, and so also, *mutatis mutandis*, with the judgments of 'softer' and 'farther.'" "Intensity as a criterion in estimating the distance of sounds," *Psychol. Rev.* XVI. 1909.

[2] Angell in "A Preliminary Study of the Significence of Partial Tones in the Localization of Sound," *Psychol. Rev.* X. 1903, 4, remarks: "The issue is really somewhat ambiguous. Richer sounds may ordinarily be judged nearer than those more nearly pure. The upper partial tones of a complex sound may be relatively more prominent when the sound is heard from a distance, and still the total sound effect be poorer and less full than when the sound is heard near at hand."

On the whole, the evidence tends to show that while, under the conditions of the experiment, position, distance, and movement marks may be very readily attached to the sounds, they are probably secondary characteristics only and arise through an interpretation of other perceived characters. Whether this is so or not, such marks certainly serve an important function in aiding identification. Indeed, in the opinion of one of us, there is no more effective or more frequently used aid to identification than the position, distance, or movement marks of a presented sound.

4. *Sounds which are perceived but not heard.*

When a stimulus is too weak to evoke an auditory sensation, nevertheless its presence is often appreciated. This of course has been noted by other workers and we made many attempts to determine precisely the character of the experience.

To S. the sound which was perceived but not heard often appeared as a temperature sensation, especially in left-ear listening during the early series. Now at this time the left ear was in a temporarily subacute condition, and possibly there may be some connexion between this fact and the thermal sensation[1]. Sometimes the temperature sensation appeared as a preliminary stage in the development of the auditory sensation proper: "In the ascending series," S. reported, "temperature sensations preceded auditory." The thermal sensation was most marked near the threshold of acuity, and was often present when no auditory experience could be discriminated: "There was a period when I heard nothing.... Then I realised that there were differences within my experience. I realised that something formless was present which was not a sound at all. It seemed to be a kind of distention of some part of the ear, but of what part I do not know. In these cases the slight temperature sensation, as if something just warm was put on the ear, was noticeable." The sensation was said to "resemble a slight flush of excitement more than anything else, as if a small sound caused an increased blood supply."

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[1] Cf. the tactile sensations experienced by B. in his subacute ear, 137.

But again the non-auditory perception of sound appeared as the upsetting of a balance. "I often," said B., "responded to something that was not a sound, but yet was something positive. Such cases are expressed as a kind of balance inside the head which seems to be chiefly motor in character, and is probably connected with a tendency to jerk the head over to one side or the other." This experience may be of the same order as the one described by S.: "I had a feeling that the sound was there, and trying to get through, but could not. The effect was that of a 'frustrated' sound"; but perhaps the epithet is here transferred from the hearing to the sound. Both of these descriptions show clearly that the experience in question is not that merely of a tendency to concentrate attention, but of a tendency determined from without[1].

A third subject, in describing the experience, used as an analogy the case of touch: "It was as if somebody should take a fine hair, and pass it over the surface of the skin. You know that there is a touch, but you can't feel it."

There is an unquestionable positive basis for this class of experiences. Response to sounds not heard was reported in twenty-three different series of trials. On only one of these occasions was there any reaction when no stimulus was presented.

When we turn to a consideration of the precise character of the experience in which a sound which is not heard is yet in some way perceived, we are at once reminded of the fact that auditory sensation may take an appreciable time to develop its full effect[2]. A certain summation of effects is required before an experience becomes definitely recognizable in terms of sound. But before that point is reached *some* effect is certainly produced, and the evidence clearly indicates that this effect may be sensory in nature. Hearing, that is to say, is really a process having a definite summation point, and only when that summation point is reached is the experience unmistakeably one of sound. The summation or blending of sensory effects, however, which at a certain point produces sound, may be expressed in other ways before that point is reached. The present evidence is to the effect that prior stages in the process may be marked either by thermal or by tactile sensibility. But under the conditions of the experiment, any such sensory experience gives rise also to a stiffening of the attention. This expresses itself on the motor side as a 'balance,' which, in B.'s case, seems to be inside the head. The suggestion then is that when an auditory stimulus is as yet too weak to give rise to auditory experience, it may produce effects referable to other sensory modes. That B. should emphasize the 'balance' factor was merely due to the fact, incident to these experiments, of a stiffening of the attention, and to the further fact that B. tends definitely to be of a motor or kinaesthetic type. We conclude then that in all cases in which a sound is perceived but not heard we have undoubted evidence of a form of synaesthesia.

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[1] Cf. *e.g.* Knight Dunlap, "Some Peculiarities of Fluctuating and of Inaudible Sounds," *Psychol. Rev.* XI. 1904, 308–18. Eight subjects examined by him responded to physical intensities after the sound had psychologically disappeared. His results are confirmed in a later article, *Psychol. Monog.* x. 1909, 16–25.

[2] Cf. Part I of this paper, 116–8.

This has an important bearing on the determination of the characteristics of a good listener. In various ways an inferential element enters into the process of listening to sounds, particularly to those of weak intensity. Clearly then the successful listener must be a person who possesses not only first-class auditory acuity, but who is able readily to appreciate the non-auditory signs of the presence of sound stimuli.

5. *The Experience of Positive Silence.*

Investigators have often called attention to the fact that the absence of a stimulus commonly induces an experience which cannot be adequately described in negative terminology. This received very definite illustration in the present experiments. One of our observers frequently noticed an effect which could be described only in the terms 'positive silence,' and which was very clearly distinguished from that accompanying the mere absence of sound.

Positive silence proved a compelling, absorbing, and satisfying experience. The subject S. was just as convinced by it that the stimulus was absent, as she was that the stimulus was present when she had perceived a sound that was definitely supra-liminal. Just as the attention aroused by the warning signal was relaxed after the reaction to a loud sound, so it was immediately slackened after the experience of positive silence. On the other hand, in cases where positive silence was absent, but the subject was unable to perceive a sound, the attention was strained either until the sound was perceived, or until the outgoing signal warned the subject of the end of the trial. It is true that under the conditions of the test, especially in the series where no accompanying signal was given, the subject after a time always became uneasy if no sound occurred, and attention was once more strained. Thus S. remarked that if there was a relatively long period of positive silence, the relaxation of strain which marked its early stages passed into a definite heightening of tension later on: "You feel that your eyes and ears are open. You hold your breath, and feel generally stretched towards the expected sound." This however was due mainly to the listener's knowledge of the routine of the experiment, the renewed strain being regarded as an intrusive and disturbing necessity, and as having no direct relation to the positive silence itself.

Positive silence possessed to an outstanding degree the characters of finality and of attractiveness. The subject wished to 'soak it in,' and to enjoy it to the full. Various writers, referring to what appears to be a more or less similar experience, all speak of it as compelling attention. Typical remarks are made, for instance, by Latzko, when he is describing the experience of a soldier home from the trenches: "There is nothing but a glorious quiet that you can *listen to* as to a piece of music! The first few nights I kept my *ears cocked* for the quiet, the way you try to catch a tune at a distance...it was so delightful to listen to no sound[1]." The phrase "a silence that could be heard" is, in fact, by no means unusual, though Titchener urges that 'felt' is the more correct verb to use[2]. It is however by no means clear what 'audible silence' precisely involves. Ward says that "the cases in which we might refer to the cessation of sound as audible are...cases of perception where consciousness when there is nothing to hear may still be auditory[3]." The suggestion appears to be that listening may continue when hearing has ceased, and may confer upon the resulting experience a positive character. In our case, however, the strain of listening was definitely relaxed on the occurrence of positive silence, and it does not appear to us that the positive character of the experience is to be correctly considered as perceptual only.

Titchener[4] has attempted definite experiments on the positive character of silence by subjecting observers for thirty seconds or more to the noise of machinery in his laboratory workshop. At the end of the set period, the noise was cut off as abruptly as possible. Various organic and kinaesthetic sensations were reported, and silence was experienced as "something *else* than sound or the cessation of sound." Our own experiments did not, of course, in any way repeat Titchener's conditions. The sounds presented were not loud, and they were of much briefer duration. The "dizziness," "change of breathing," "oppression in the region of the drumskins," "boring in the ears," and so on, reported by Titchener's observers, were doubtless due to the fact that his experiments involved the abrupt cessation of a loud noise. No such marks were present in our case. Nevertheless, we also observed that silence is "something else than sound or the cessation of sound."

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[1] *Men in Battle.* By Andreas Latzko, Eng. trans. 1918, 19–20: italics, own.

[2] *Journ. of Phil., Psychol.* etc. xiii. 114.

[3] This *Journal*, viii. 1916, 216.

[4] *Op. cit.* 114.

For an explanation it is natural to turn first to the facts of contrast, and to explain positive silence as a heightened contrast effect. But by itself such explanation is inadequate; for what really calls for further analysis is the mode by which the contrast is made so peculiarly effective as to give rise to the experience of positive silence. In point of fact, contrast, in one form or another, comes in very frequently indeed throughout the whole course of experiments of the kind here reported. Consequently if, on occasion, contrast is found to be accompanied by so markedly new an effect as that of positive silence, such contrast obviously calls for some special analysis.

One character of the experience was very constant and arresting. Positive silence always appeared with notable suddenness. It did not always occur immediately upon the cessation of sound. But even when it was interpolated into the midst of a long empty-appearing interval, as often happened during the two-hour sittings, it came with extreme suddenness. In those tests in which a signal was used, the listener repeatedly reported positive silence immediately after the appearance of the warning signal, and the experience apparently took no appreciable time to develop.

Now it has several times been observed, both by us and by other investigators, that, in listening, the outgoing of a stimulus is more definitely and sharply marked than its initiation, much as in electrical conduction a break in contact is cleaner than a make. The suddenness of development of positive silence suggests that it is the result of the cessation of some sensory experience. But it is at the same time more than a positive *perception* of cessation. Change abruptly experienced very commonly indeed induces an emotional or feeling state, and when a sound which is *expected* is not experienced, the emotion is all the more likely to occur[1]. Expectation is not essential, indeed, for positive silence occurred sometimes in intervals between the signals. In these cases it may be suggested that there is a momentary suspension of some continuous excitation, possibly in the inner ear. When the situation is such that expectation also comes into play, the effect of this momentary suspension is the more marked. But in either case, the positive character of the silence experience, under the conditions of the present experiment, appears to be contributed largely by an attendant feeling or emotional factor, and not to be purely perceptual in nature.

It is not urged that this emotional character constitutes the whole of the experience here called positive silence. Were this the case, it would be possible to obtain an experience precisely similar, even when stimuli other than the auditory were being employed. There is good reason to believe that an experience which is emotionally similar can be obtained, but a part of the total result is contributed by the changes in the functioning of the *particular* sense organs involved. Further investigations are needed to show definitely, for example, how the total mental reaction to the cessation of light differs from that occurring in the case of positive silence.

6. *Subjective Sensations.*

The existence of false responses, together with their necessary accompaniment, the subjective sensation, has already been demonstrated. But it is of some interest to consider them further solely with reference to the observers' remarks concerning their nature.

The tendency towards subjective sensations may no doubt have been increased by the conditions of the experiment, in which sounds were produced at more or less regular intervals. Thus S. once remarked: "After a certain number of sounds which were easy I noticed regular pulses of attention. Then it was difficult to refrain from responding to subjective sensations, when the time came for directing attention to the sound." Again B., whose natural type of reaction is muscular rather than sensorial, found that merely having his thumb and finger on the reaction switch predisposed him to subjective sensations: "I found it a good plan, during an unfilled interval, to take away my finger from the switch. This dispelled any tendency to subjective sensations which I might have had." To B. it seemed that subjective sensations appeared more readily in the dark.

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[1] On some occasions such absence may even give rise to intense fear. This was the case in some experiments carried out by one of us on guinea-pigs, where light and sound stimuli were simultaneously presented. After prolonged experience with the combined stimuli, the use of the sound stimulus was discontinued, the light alone being presented. In every case, the animals at first exhibited symptoms of the greatest fear, and the reaction time was enormously reduced. Even the experimenter, although controlling the situation, and hence in some measure prepared for it, always experienced a certain amount of shock the first time that the light was presented without its associated auditory stimulus. The experience was more startling than was that of the actual sound itself.

Although both observers were sure that their experience was properly sensory, there was no evidence that it could be considered as analogous to visual after-sensations. On the whole, relatively loud sounds were less likely to be followed by subjective sensations than were those of weak intensity.

It is interesting to consider whether with practice a listener can distinguish between subjective and objective sensations with any degree of accuracy, and on what the attempt to do so is based. B. reported nineteen sittings in which he was much troubled by subjective sensations. During these sittings he responded to forty-eight catches in all. On seven occasions he definitely reported "no subjective sensations." These seven sets of trials produced nineteen responses to catches. This gives only the very slightest support to the suggestion that when the subject is on the look out for subjective sensations his response is likely to be more accurate. S. reported strong subjective sensations twelve times, and during these twelve sittings responded twenty-one times to catches. Only twice did she definitely assert that she had no subjective sensations, and on one of these occasions she responded six times to catches. Again we receive but a very slight support to the suggestion that a listener who is on guard against subjective sensations may be somewhat less likely to respond when they occur.

The basis of such discrimination needs further investigation. B. often relied on the perception of the oncoming of the sound: "A subjective sensation," he said, "has no exact point at which it breaks into the silence." S., on the other hand, found the outgoing of an objective sensation easier to notice than its incoming[1]. Consequently she would note the outgoing of a sound of a particular intensity, and then endeavour to identify the sound when it reappeared. As the character of subjective sensations was usually constant for a given set of trials, this plan appeared to work fairly well. Another observer used the same device: "Sometimes," he said, "I felt there was no sound, and then I noticed when it stopped, and knew that there had been something. The absence of the sound was more noticeable than its presence."

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[1] Knight Dunlap, *Psychol. Rev.* XI. 1904, 308–18, reports that a sound so physically weak as not to be heard may yet be heard to stop. He states further that the stoppage of a minimal sound is more easily observable than its starting. S. confirmed this observation on a large number of occasions.

It should further be remarked that in the two-hour sittings we found far less lag in the reaction to the outgoing of very weak sounds, even when they had continued for five minutes or more, than to their incoming.

The fact that the outgoing of a faint sound is frequently noticed, even when the sound has not been heard, in some instances leads to response after the stimulus has ceased. This fact undoubtedly accounted for a certain proportion of the 'false' responses in the tests where no signal was used.

B. once reported: "I find that the subjective sensation appears to be further inside the ear than the objective sound. Also this morning subjective sensations were very distinctly higher in pitch and different in quality." But the appearance of such differences cannot always be relied upon.

It is thus clear that subjective sensations definitely occur, that they may possess all the vividness of actual sounds due to external stimulation, and yet that by careful scrutiny they may to some extent be prevented from producing catch responses.

E. THE CHIEF DETERMINING FACTORS OF THE REACTIONS.

1. *General Determining Factors.*

The analysis of the character of the sound experience has already demonstrated that, under the conditions of the present experiments, the response of the subject may be determined by many factors other than that of the bare perception of the sound itself. It is interesting to consider these factors in detail, to study the manner in which they function, and to enquire at what period in the course of the experiments a given factor attains its greatest influence.

We shall consider first the evidence of the direct reports furnished by the observer B.

(i) In the early stages of the test this subject tended to confirm his perception of the sound by vocalisation, and not to react until such confirmation had been given.

"There is," he said, "always a tendency to vocalise during the listening period. Having heard the sound I feel inclined to say: 'Yes, there it is.'" But a month later the observer reported: "The vocalisation which accompanied the early stages of the test has almost entirely disappeared."

(ii) At the beginning the response was partially determined by an awareness of the nature and extent of muscular contraction. "The amount of muscular contraction present," said B., "seems to be considerable. There is an impulse which starts in the ear, and seems to travel across the shoulder and down the arm. This may be fanciful, but I have an impression that something is going on, and all the time there is consciousness of movement in the throat. These things all come only when there is a sound." And again: "When the sound comes, first I pinch the switch, then my fingers get tense, then the switch is pushed over."

The awareness of muscular strain became far less prominent with practice, and in so far as it remained it ceased to be widely distributed, and was localised simply "in the head."

(iii) Variations of mood, or of conscious attitude, played a large part, particularly in the early stages of experimentation.

References to moods appear very frequently indeed in the reports, and the moods themselves are very varied: "When I get an unmistakeable sound, something seems to happen at the back of the head. There is excitement, and a feeling of sudden release, but no consciousness of making up my mind. If two or three sounds of which I am certain follow one another, I get a feeling of confidence, a pleased feeling, a feeling of doing well. This does away with the strain, and also with the expectation of what is coming next. When I am uncertain, no response can be taken as complete in itself. As soon as one trial is over I am looking forward to the next....Sometimes I make a response just because I want to respond. This particularly occurs after several cases in which I feel tolerably sure a sound was present, although I am not able to hear." Again: "The whole of my attitude is dominated by a desire to be able to hear, and if I don't hear the desire is carried over to the next trial. If I do hear, I am more satisfied, and in a more favourable position for the next trial. The feeling of certainty appears to follow on a consciousness of muscular contraction in the shoulders, arms, and throat."

Far more than is the case with the other factors noted, the conscious attitude may vary its character within the limits of a single set of trials. It seems, however, to be very readily determined by something that occurs towards the beginning of a series. Seeing that conscious attitude may certainly exercise a definite determining influence upon the character of judgments passed, it may well be that the fact that the mood is frequently set up during an initial period may have something to do with the dominant effect of the first few of a series of judgments. It has often been remarked that when a number of judgments have to be made successively the character of the first two or three is very apt to set the direction of all of the others. Suppose some early judgment is chiefly determined by a mood or attitude having a marked accompaniment of feeling-tone. When the perception of a situation gives rise to feeling, even when the situation changes somewhat, the feeling often tends to remain. Further, feeling which properly attaches to certain definite cognitive details easily spreads over associated details. Moreover, what is thus true of feeling-tone is true, in precisely the same sense, of the mood to which the feeling may be attached. Thus if an early judgment in a series is largely determined by mood, the persistence and spread of the latter readily account for the fact that successive judgments tend frequently to be like the first. B. once reported: "My mood was somewhat reckless, and I responded to more than usual. This I believe was started by a response early in the series. Then, more or less involuntarily, I replied to what I immediately realised might have been a catch. As I was not sure, however, I simply fell into the attitude: 'Well, I might as well keep it up'."

Except under conditions which will be considered later (see pp. 160–2) the conscious attitudes became less prominent factors in the determination of B.'s reactions as he became more practised in the task of listening.

(iv) In the case of a continuous sound, differences within the sound experience itself, at different points of its presentation, often determined the response.

"There are two maximal points for the sound," said B., "first the moment of onset, and second a moment or so after the onset, when the sound appears to swell out."

(v) The length and frequency of empty-appearing intervals were important from first to last.

This factor was particularly appealed to in the cases of uncertainty as to whether a sound were present or not: "What constituted the difference between cases in which, in spite of uncertainty, I did respond, and cases in which I did not, I can't say for certain, but I think whether I have been hearing sounds or not just before is important. When I get a fairly loud sound, followed by a pause, and then a sound which I am not able to identify, I respond simply on the ground that it probably is the sound, because the long pause suggests considerable adjustment of the apparatus[1]. It seems to me that it is this sort of unexpressed reasoning which has a good deal to do with whether I respond to the uncertain sounds or not."

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[1] The experimenter always endeavoured to make the intervals of presentation as irregular as possible, so as to counter this tendency on the part of the subject.

(vi) At first no response appeared purely mechanical, but after long practice the process of reacting occasionally quite escaped attention.

It is interesting to trace the development towards automatization throughout the reports. Very early B. said: "I could not clearly determine the conditions of the response, but am convinced that no reaction was purely automatic." Then a little later came the remark: "In the case of sounds heard very distinctly, the response appears almost automatic. But there is some kind of factor present which it is very difficult to catch or to describe." A month later he said: "Two or three responses were absolutely automatic, and I knew nothing about them till they were over." To the very end, however, after fairly constant practice extending over six months, B. remained doubtful whether complete automatization was reached. One of the latest reports states: "I was thinking of other things. I find however that though it appears that the whole process goes on mechanically, yet whenever a sound—even a loud sound—comes in, there is a swinging back of the attention. So I don't think it is a perfectly automatic response."

(vii) Occasionally a tendency to rhythmic response was aroused.

"At one period," said B., "I got into a regular rhythm of response." This tendency appeared to be a mode of reaction more or less specific to a particular sort of mood: "When one gets into a dreamy attitude, it appears to be a kind of rhythm which determines the response. One alternates from the case of responding to most things, to that of responding to nothing unless one is dead sure, and so back again. This sort of condition is unfavourable to listening." The concluding remark of this report appears to be well justified, and is borne out by a study of the results. The tendency to rhythmic response leads both to false reactions, and to a high threshold.

Taking all these different factors, (i) and (ii) were operative at the beginning of the experiment; (iii) was present throughout, but was chiefly important in the early stages; (iv) and (v) were present from start to finish, and were, if anything, more important in the later stages; (vi) came only after considerable practice; (vii) was not present to any great extent, but tended to be more important in the later than in the earlier stages.

When we turn to S.'s reports, we find that very much the same factors are referred to, but that they are not by any means arranged in the same order, either as regards time or apparent importance.

(i) There was throughout no reference whatever to vocalisation.

(ii) Awareness of muscular strain entered a month after the onset of the experiments. Even then its chief function appeared to consist in the determination of mood or attitude, the latter being directly related to the response. Also S. was never conscious of strain localised in the shoulder, arm or hand, but only in the head, ear, eye, and, in the two-hour sittings, in the neck.

(iii) Moods and conscious attitudes appeared as determining factors from the beginning, and persisted as such till the end.

S. had a far greater wealth of mood than B., and her reactions also appeared to be more readily determined by the attitude factor. In her second sitting she said: "I am sure that the responses are determined largely by a feeling of confidence, or of diffidence. Hence after a loud sound I reply the more readily, because I feel buoyant then." Again remarks such as: "When a moment of acquiescence becomes a moment of conviction, the response comes automatically," were frequent. And at the end of the experiments the importance of attitude continued: "I was very eager to respond to all sounds. As a rule, I took the least hint of a sound, and responded without deliberation. There was a pleasant absence of boredom, and a general sense of well-being."

On the other hand, it is certain that S. experienced great variety of moods which appeared to produce no effect on the results. The most striking illustration of this occurred on a day on which a somewhat restricted range of fairly weak intensities was presented. Throughout the whole sitting the subject had very few sounds of which she could feel in the least degree certain. Her report described the moods thus induced in great detail:

"The strain was terrific. There was a period during which I heard nothing, and wondered if I was going deaf. I then realised that a formless something was present which was not sound. At the same time I felt as if nothing really was present. I gripped the table, and presently got used to the experience, and imagined I was having subjective sensations only. Then I got a fit of absolutely choking rage. I had a feeling of rigidity all down my hand, and my fist was clenched....This lasted for a while, and I could not attend at all. There was a most extraordinary feeling of being thwarted, accompanied by a horrible feeling of hopelessness which took all my attention away from the sound, and would have prevented my hearing it anyway....The alternation between hopelessness and more or less indifference came frequently. Sometimes I did not know when I was listening, and when not. For the first time the consciousness of my own breathing seemed to hinder listening. The fact that I was angry with what I could not alter, intensified the anger."

In spite of all these apparently unfavourable moods, the observer's performance on this occasion was approximately normal (cf. pp. 156–7).

(iv) S. never reported fluctuations of the intensity of sound while she was listening, and found little or no help in attempting to note the onset of sounds. From the first the factor of recognition was important, and in this respect she repeatedly referred to the help afforded by the outgoing of a sound (cf. p. 149).

(v) S., like B., was influenced by the length of the pauses, and the effect of earlier sounds. These factors produced an attitude of expectation in which the observer prepared to receive a sound of a certain intensity. In S., as in B., the length of the pause and the character of the just-presented sound exercised their chief importance in the later stages of the experiment.

(vi) S. settled down to a mechanical mode of response much more quickly than B. In the first month she reported: "I merely had to wait, and something inside decided the matter for me." And then a little later: "My responses were almost wholly automatic. Sometimes I wondered why and to what I was responding, and yet I was impelled to respond." Later still, the same experience was repeated with curious vividness: "It seemed to me that somebody else was doing the responding. I felt quite emotionless to this other person, whose hand I definitely visualised though I did not see my own hand, and whose antics in responding to the sound struck me, the detached onlooker, as supremely ridiculous and puerile....I was in no way called in as arbiter, and had no personal responsibility to the sound."

In S.'s case, in particular, the automatization of the response appeared to produce a lower threshold, and more accurate results. Of the seven occasions on which automatization of response was reported by S., her performance was four times distinctly above normal, and never below normal.

(vii) The tendency to rhythmic response was occasionally present, but was not at all prominent. "After a certain number of easily heard sounds," S. reported, "I noticed pulses of attention, and found it very difficult to refrain from responding, when the time came for the sound."

When we turn from the description of the determining factors of the reaction to their analysis, two interesting questions at once arise: (*a*) how do such factors come to be present at all in experiments of this type, and (*b*) in what way do they exercise their influence? These two questions may best be discussed together.

Every one of the factors just described is either itself of the nature of a habit, or else tends to give rise to a habit. But while some of the habits take their rise within the limits of the experiment itself, others are preformations carried over to this special set of conditions from other contexts. The second type is illustrated by the factors dominantly present in one observer, and absent, or almost absent, from the other. The vocalisation, for example, which seemed valuable to B., was never found in the case of S. B. is, in fact, so dominantly of a vocalising type that, whatever the task he may have to perform, there is a strong tendency for inner speech to occur and to exercise influence on the determination of the reaction. The same is true of the factor of muscular strain which was relatively prominent in the case of B. but relatively slight in the case of S.; while S.'s greater wealth of mood must undoubtedly be referred to intellectual and affective habits which go far beyond the limits of this experiment.

Two remarks may be made in regard to factors of the second class. Either they tend to be chiefly effective in the early stages of the acquisition of new modes of reaction and to disappear later; or else, if they persist, they lose their important determining influence on the reaction. The first is precisely what might be expected to happen, and is illustrated by the vocalisation and muscular strain factors in the case of B.; the second is particularly interesting, and finds illustration in certain of the attitudes in the case of S.

An attitude may be the direct outcome of some specific character of a presented situation. In this case it undoubtedly largely determines the nature of the response made to that situation. But it may equally be the outcome of the *general* nature of the situation, and then it appears that the attitude is a less efficient determinant. For example, an unmistakeable sound always tended to set up in B. an attitude of confidence which certainly made him far more ready to respond. But the succession of apparently unfavourable moods set up in S. when a series was presented containing but very few readily audible sounds had little or no effect on the nature of her reactions. The former was a mood induced by the specific character of a particular occurrence, the special loud sound in question being an outstanding feature of the external conditions of the situation to which reaction was demanded. The latter, on the other hand, was not an attitude induced in this manner. There was no particular prominent detail of the presented situation which could be clearly indicated as having given rise to the mood. What was present was not so much a feeling of satisfaction or dissatisfaction as regards any particular response, but rather a general feeling of ill-success, such as might equally well have accompanied any situation in which reactions of a hesitating and tentative character had occurred.

A further suggestion may be made. Probably all, and certainly most, conscious attitudes possess to a marked degree the capacity for transference[1]. That is, when a mood has been evoked by some specific occurrence or situation, *whatever* occurrence or situation follows more or less nearly often tends to be accompanied by a mood of precisely the same character. There is no doubt whatever that many of the moods reported both by S. and by B. in the course of these experiments were only to a slight extent attributable to the actual nature of the experiment during which they occurred. Supposing the latter had been of a very different type, provided it had been inadequate wholly to absorb attention, very much the same series of moods would in all probability have been experienced. It is tempting to suppose that when the moods which surround a series of reactions of a relatively mechanical nature are of a transferred character, they have little effect in determining the nature of the reactions. This is clearly a matter of some importance, seeing that there are no events and actions of daily life but have their halo of moods. Further investigations are called for to demonstrate precisely when, under normal conditions, the conscious attitudes are real determinants of action.

Meanwhile the importance which moods certainly possess under some circumstances makes it yet more clear that no matter how simple may be the form of response required from the human subject, the analysis of the character and efficiency of the response often demands reference to factors the history of which goes far beyond the limits of the definite conditions of the reaction in question.

When we turn to the habitual modes of reaction set up within the limits of this experiment, we find little that calls for remark. They were all based on a knowledge of the conditions of the experiments, and illustrate clearly the inferential character of much of our response to sensory stimulation. Factors of this type were found in both observers, they were brought into play somewhat less readily, but were definitely determining influences to the end.

Obviously the different determinants cannot all have operated in the same manner. Vocalisation, for instance, was confirmatory; but motor experiences were anticipatory. Moods, being persistent elements, usually confirmed one response, and in so doing helped to direct another by anticipation. Both the 'transferred' mood, and the mood determined by the *general* character of the presented situation seemed to have anticipatory functions from the beginning of a group of tests. Rhythm was clearly anticipatory, while the occurrence of maximal points in the sound experience, and the length of intervals, played their parts chiefly by producing expectation.

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[1] Transference must be distinguished clearly from the persistence and spread spoken of on p. 152.

The anticipatory character of motor sensations may probably be set down to the effect of a sound before it is heard[1]. In this respect it is interesting that much of the strain of which B., the vocalising subject, was conscious was localised by him in the throat. It is as if there may be an incipient vocalisation even before the sound is at all clearly experienced. In point of fact it appears highly probable that we have here come upon a type of determination by sensory experience which is very frequent in everyday life. Very many of the extremely swift reactions of daily life must surely be due to the fact that sense stimuli produce definite, but normally unnoticed, effects before they give rise to their full result. The anticipatory function of the mood however can hardly be explained in this manner.

What the whole of this study of the determining characters of the reaction most clearly shows, is that many factors other than those of sensory acuity and adaptation help to determine reaction to sounds of slight intensity. We are confident that the same result would have been brought out, had the investigation been directed upon any other sensory mode. It is a truth far too often forgotten, but abundantly illustrated by this attempt at a qualitative study of the factors which enter into listening, that no absolute measure of any mode of sensitivity is possible. The sense response is, even in the very simplest cases, carried out amid a mass of conditions not themselves wholly reducible to sense terms. Precisely what these conditions are depends on the specific character of the experiment under which the response is made. And not only is any measure that may be secured relative to the conditions of the experiment, but it is relative also to the whole attitude and preformed habits of the responding subject[2].

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[1] Cf. pp 143–5.

[2] The point is well brought out by S. W. Fernberger, "The Effect of the Attitude of the Subject upon the Measure of Sensitivity," *Amer. Journ. of Psychol.* xxv. 1914, 538–43. "One cannot measure sensitivity in absolute terms, one can only say that a given sensitivity has been found to exist *under certain given experimental conditions*.... Inasmuch as

2. *Effects of Specific Changes in External Conditions.*

(i) *Light* versus *Darkness.*

We made some attempt to determine the external conditions for listening that are conducive to the best results, and definitely varied some of the factors that seemed of importance. The first question that arose was as to whether an observer's reactions may be affected favourably or unfavourably according as the tests are carried out in a dark or lighted room.

We found that both S. and B. preferred to listen in a light room, but that in the case of B. very bright illumination was not pleasant. S. preferred light from the first, but B. was at the outset convinced that darkness, which appears to favour concentration, was the better condition. However, after several trials, he reported: "There is no question about it: I prefer the light. In the light I feel greater ease and comfort, and the strain is less." S. was of the same opinion: "In the light there is much less strain. My attitude is passive. The ability to look round and notice things in the room prevents the feeling of strain, without hindering the response." When listening in darkness followed the series of coloured lights, B. was inclined to revert to his earlier decision: "My judgment is that after all darkness is the best of all conditions for listening. The thoughts and ideas that come have nothing much to do with your immediate surroundings, and consequently from the point of view of listening are less distracting. I suggest that either excitement or depression produced by sense experience of some other kind is unfavourable." A third observer independently reported: "With the light I have a different background of attention. All the room, and the things in the room are open to me. This distracts my attention, and I do not hear so well as in the darkness." But S. was of one view throughout: "I don't like the darkness, which distracts my attention."

When an observer was listening in the darkness, there was a greater tendency for his reports to be concerned with an analysis of the characters of the sound, and of the reaction[1]. This may perhaps suggest that during the early stages of learning to identify a sound, darkness is a

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we are dealing with the total psycho-physical organism, and not merely with sense organs, it is after all not surprising that the attitude of the subject should have a profound effect upon this sensitivity."

[1] Cf. David A. Anderson, *Psychol. Monog.* XVI. 150–6, who also points out that to his subjects, while the quiet and freedom from distraction of a silent dark room were soothing, they also served to make the observer more critical.

favourable condition. But when once the sound has been learned, light should replace the darkness, since in the light most observers are probably more at their ease, and more completely comfortable.

(ii) *The Effects of Varied Colours.*

We varied the colour of the illumination in the observer's room, largely with a view to getting further information concerning the possible effects of mood or conscious attitude. By this time both observers had become thoroughly practised, and it has been mentioned already that one of the effects of growing familiarity with any situation demanding a certain type of reaction is that conscious attitudes tend to become much less marked. But with the variation of colours in the room, moods and attitudes were revived in all their vividness. Thus with red S. "feels indifferent"; was "conscious of being weighted down from without"; had "a feeling of complete hopelessness"; while B. "feels stimulated"; had "a mood of 'it doesn't matter much if I have made a mistake'"; possessed "a sense of freedom." With green B. "feels depressed," but S. had "a mood of repose and steadiness." Blue made B. "dissatisfied on account of the coldness of the light"; while S. experienced a "sort of excluding influence, unfavourable to the play of fancy"; and again, felt "restful, calm and soothed." Violet gave S. "a jaded impression, a stale feeling," and induced "an artificial feeling attitude." Later she reported of violet: "I hate this cold light. It gives me a feeling of confinement, and every now and then an asphyxiating feeling, and an attitude of general mental and physical stuffiness." To S. the yellow light was "stimulating," but it produced "nauseating suggestions." B. regarded the yellow as "detestable, bright but not stimulating, and I felt as though I wanted to get away from the yellow all the time."

On the whole there is considerable consistency of mood for the same person, for a given colour of light. Though the order in which the colours were used was varied, the attitudes induced remained very much the same. The suggestion is, then, that the moods in this case were actually induced by the colours of the illumination. Probably *any* outstanding change of external conditions would tend to revive the influence of the conscious attitude. Supposing this to be true, we have here illustrations of attitudes directly induced by a specific character of a presented situation. And if the suggestion made earlier is to be substantiated, we should expect to find evidence of definite influence of attitude upon response.

The experimental data are not unambiguous, and it cannot be expected that they should be, since moods are only one set of determining factors of the response. But on the whole they confirm the suggestions already made. For example, in green light, which he found "most depressing," B.'s results were uniformly bad; while with the same illumination, which she found "restful and steadying," S. did unusually well. Blue light, which displeased B., was associated with a very bad performance, and a large number of catch responses; while S., who considered the blue "most pleasing," obtained good results. On the other hand, B. found the violet light pleasant, and obtained results which were better than usual; but S. was greatly disturbed by this illumination, and her performance was below average. Finally S. did well in yellow light, which she found "stimulating"; while B., who found the illumination "detestable," did badly. On the whole, therefore, the suggestion made already tends to be borne out by a study of the results with vari-coloured illumination. When a mood or attitude accompanies the carrying out of a certain task, that mood or attitude being the direct outcome of some specific character of the situation which is present, the efficiency of the response tends to be definitely determined, among other things, by the character of the mood.

Another effect which may be attributed to the variable colours of the illumination may be noticed. Both observers reported the presence of imagery of various kinds throughout the whole course of the experiments. S., who usually felt a need of some slight distraction, seemed somewhat more prone to irrelevant imagery than B. Normally she listened with closed eyes, but in order that the colours should be allowed a chance of producing their greatest effect, she was told to keep her eyes open when the coloured illumination was used. The need for distraction and the necessity for keeping the eyes open, seem together to have produced a very great wealth of imagery, which now became predominantly visual. The most fantastic shapes of people, animals, and occasionally of plants, were seen upon the walls. To some extent, B. shared in this effect of the colours, and he also experienced far more than the normal amount of imagery. But so far as could be observed, the great wealth of irrelevant imagery produced absolutely no disturbing effect upon the reactions to sound. Its impotence may, in fact, be compared with that of the wealth of attitudes already described to have occurred on one occasion in the case of S. (see p. 154). Transferred moods and irrelevant imagery are both perfectly normal occurrences, and only in those instances in which the mood or the imagery is definitely attributed to the character of a present situation, that is to say, where the subject is entirely unconscious of their irrelevance, do they seriously disturb the normal reaction.

(iii) *The effects of external distracting sounds.*

Research on the effects of external distracting sounds was conducted chiefly with that modified form of Politzer's acoumeter test which we have called the boxed acoumeter[1].

The distracting stimuli generally used were: a whistle blown continuously, Stern's tone-variators adjusted for pitches ranging from 200–1000 v.d., an electric bell and a buzzer. One or more of these stimuli would be kept in continuous action for periods varying from 3 to 45 minutes, during which they formed a background of sound against which the acoumeter clicks were given.

All the subjects who took part in this test were well-practised and reliable.

The degree of distraction caused by such stimuli seemed to depend on (α) their intensity, (β) their pitch, quality and uniformity, (γ) their affective character. The first was by far the most important determining factor, any sound, if sufficiently loud, enormously raising the subjects' threshold for auditory acuity. Four different ways in which the loud sounds rendered the subjects insensitive and unresponsive to the acoumeter click were noted. These were: by masking or obliterating the sound, by distracting the subject's attention from the click, by rendering the click unfamiliar so that the subject no longer recognized it, and by causing a condition of irritability in the subject which unfitted him for concentrated attention. Sounds which normally occasioned no disturbance were found to produce a pronounced effect if their intensity was increased. Moreover, within fairly wide limits, a direct relation was found to hold between the intensity of the distracting sound in question and the threshold of the subject under examination.

Certain kinds of sound were found more distracting than others, irrespective of their intensity. For instance, in most cases the electric bell was found much more disturbing than the buzzer. The whistle also was observed generally to exercise a most disturbing effect; this was usually attributed to its 'piercing' character. The tone-variators produced remarkably little disturbing effect with the particular pitches used. In some cases even, their use appeared to have a slightly favourable influence upon auditory acuity. This may have been due in part to the greater stability of attitude induced. Thus, it appeared that the presence of continuous sounds of this character interested the subject, and so proved unfavourable both to reverie and to discursive thought[1]. Distractors of this class served to keep attention more or less fixed and thereby ensured a certain constancy of mood: at the same time they rapidly produced a high degree of habituation which prevented them from becoming all-absorbing[2]. A second and more direct effect was also noted, for it seemed as though this particular sound-background in some way served to throw the click into relief. The same result was often observed when clicks in the neighbourhood of the threshold occurred simultaneously with the striking of the hour by a town-clock. Facilitation in this case is the more remarkable in that it occurs when the distracting stimulus is unexpected and, in a sense, novel; under such conditions the distracting effect would be expected to be particularly marked.

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[1] Part I of this paper, 103.

Musical tones certainly proved to be less distracting than noises. But the beats of two simultaneously sounding tone-variators, set at 222 and 225 v.d. respectively, raised the subject's threshold in several cases. That the beats were the disturbing factors in this case was rendered clear by the fact that when the tones were coincident very little effect of distraction was observed. These slow heavy beats closely resembled the throb of an engine. In these and similar cases it is difficult to determine whether the distraction effect is due entirely to the intermittent character of the stimulus, or whether, as seems likely, qualitative differences enter in to some extent. Cassell and Dallenbach regard the temporal character of the distractor as being of fundamental importance, and consider qualitative differences to be practically negligible. Our experiments, while they confirm the importance of the continuous or intermittent character of the distracting stimulus, and in the latter case of its regularity or irregularity, also seemed to point most definitely to the considerable part played by the quality of the stimulus. Thus continuous stimuli were found to produce the most diverse effects which could only be satisfactorily explained as due to differences of quality. The effects produced by different kinds of distractors were found to be fairly constant in several cases over a relatively wide range of intensities.

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[1] Cassell and Dallenbach note a similar effect. Dallenbach on one occasion reports: "I seemed unable to inhibit certain associative trains during the control series, whereas they were inhibited more or less involuntarily during the distraction series." "The effect of auditory distraction upon the sensory reaction," *Amer. Journ. of Psychol.* XXIX. 1918.

[2] In this respect the tone-variator background may be compared with the use of coloured light in series III (see pp. 160–2). Cf. also S.'s attitude in a lighted room (p. 159).

In our experience the most distracting sounds, as a rule, were those possessing human interest (as, for example, a footfall or the whirr of a taxi-cab) and unfamiliar noises or sounds, the source of which could not readily be assigned.

The well-marked disturbing effect produced by the sound of the wind is probably to be attributed mainly to its irregular character; in addition, it generally produced a pronounced degree of irritability with a consequently decreased power of concentration.

Adaptation was rapidly effective in the case of most continuous distracting sounds, provided that they were not too intense. Indeed, with the tube-and-screen acoumeter, the employment of a background of continuous buzzing noise was found to give more reliable results because it shut out irregular accidental noises which would have been far more disturbing. Irregularly interrupted sounds proved highly disturbing even when repeated so often that the subject was well accustomed to them. Of external sounds of weak intensity, much the most disturbing were those closely resembling the stimulus sound in pitch and quality. Their presence quickly occasioned loss of confidence in the subject, and his reactions soon became markedly erratic under these conditions. In the tube-and-screen test the twittering of one particular bird outside affected the subjects very considerably. Its note closely resembled the click of the acoumeter, and subjects repeatedly affirmed that it "put them out" and made them extremely uncertain. No other normal, accidental distracting sound proved so troublesome, or produced so diffident a mood in the listeners.

Lastly, the disturbing effect of external sounds was found to be determined in no small measure by the subject's personal relation to them. Apart from the universal effects of intensity, quality, duration, etc., just mentioned, the aggressiveness, the pleasantness, or the indifference of the distracting sound seemed not infrequently to depend on factors peculiar to the individual, and in particular to the subject's temperament, or to association arising from his past experience.

An attempt was also made to determine the subsequent effect of prolonged loud distracting noises on auditory acuity. This was found to be much less than was anticipated. But in the severest trial, where for twenty minutes the subject sat listening to a loud electric bell ringing close to one ear, while a whistle was blown near the other causing a deafening noise, a considerable degree of insensibility to the acoumeter click was shown in the tests immediately following the cessation of these noises. Nevertheless recovery rapidly took place, and within from five to ten minutes the acuity became approximately normal again. One effect of such continuous distracting noises was to make the subject more fatigued at the end of a sitting, even though his response had remained apparently unaffected by the background of sound. Such fatigue is probably due mainly to the increased excitement or increased strain demanded.

F. The Observer's Judgment as to the Efficiency of his Reactions.

Both observers often attempted to estimate the value of their results. Comparing judgments with performances we get the following record:

B.'s Judgments.	B.'s Performances.
Bad.	Below normal.
Good.	Normal.
Bad.	Above Average.
Bad.	Good.
Normal.	Normal.
Good.	Normal.
Bad.	Bad.
Normal.	Normal.
Normal.	Normal.
Below normal.	Below normal.
Bad.	Below normal.
Above normal.	Below normal.
Bad.	Good.
Below normal.	Normal.
Below normal.	Normal.
Good.	Good.
Bad.	Bad.
Below normal.	Below normal.
Bad.	Bad.

S.'s Judgments.	S.'s Performances.
Good.	Above normal.
Below normal.	Normal.
Bad.	Above normal.
Good.	Good.
Below normal.	Normal.
Bad.	Below normal.
Bad.	Below normal.
Below normal.	Below normal.
Below normal.	Good.
Good.	Below normal.
Below normal.	Normal.
Normal.	Above normal.
Good.	Good.

The order of the tables corresponds to the order of judgments made by each observer. It will be seen that B. has 47 per cent. and S. 23 per cent. correct judgments. There is a slight suggestion that B. is improving with practice, but no such improvement can be detected in the case of S. Possibly a 'good' judgment is more likely to be correct than a 'bad' judgment; for whereas only 27 per cent. of the 'bad' judgments are correct, 42 per cent. of the 'good' judgments are confirmed by the corresponding performances. Moreover while two 'bad' judgments are correlated with good performances, and two more with results above normal, no single 'good' judgment is passed on a bad performance, and one only on a result below normal. The suggestion is that while an observer generally knows when he is doing well, his belief that he is doing badly may very likely be unfounded. This is interesting in view of the fact that the judgment as to good or bad performance is commonly based on the conscious attitude of the listener. Possibly the attitudes which are felt as positively favourable have a more definite effect on the results attained than those which appear as uncomfortable and disconcerting. It may be that a compensatory effect, in the form of the exertion of extra effort, occurs in the latter case, and is not allowed for by the listener.

G. SUMMARY.

The present work has developed from an attempt to devise and apply a series of tests for the selection of candidates for the Anti-Submarine Service during the war. One set of these tests concerned auditory acuity, and the aim of the research herein described was (i) to devise apparatus for and methods of application of a reliable auditory acuity test, using a continuous source of sound, and (ii) to determine the optimal conditions for listening to sounds of weak intensity. The continuous sound was used in order to approximate as nearly as possible to the conditions of submarine listening.

Earlier observations regarding the unreliability of drop methods of producing sounds were confirmed, and in most of the experiments the sound was electrically conducted, its intensity being expressed in terms of the strength of current used. At first an ordinary buzzer, later an electrically maintained tuning-fork, was used as a source of sound. With the setting employed, all trace of a warning click in the telephone receivers was eliminated, and the methods developed show that results with a continuous sound may be as definite and reliable as those secured by the more commonly used single intermittent sound.

The chief results obtained show:

(*a*) that variations are liable to occur in the relative efficiency of the two ears, such variations as were observed developing gradually and extending over a long period;

(*b*) that under the conditions of this experiment binaural listening appears to be more efficient than uni-aural;

(*c*) that at the same time the relative acuity of either ear, or of both, appears to be affected by the order of testing, the tendency being for the first threshold taken on any given day to be the best;

(*d*) that for a practised listener the condition which most favours a low threshold and few false responses is the employment of a small range of finely graded stimuli restricted to the neighbourhood of the threshold, and that usually the best period of listening is the initial period;

(*e*) that the order of presentation of the sound—*e.g.* of regular descent, of regular ascent or of mixed intensities—affects different observers in a different manner, so that no one method can be said to be universally the best;

(*f*) that sounds of weak intensity may take as long as four seconds to produce their full effect;

(*g*) that the use of a warning signal in experiments on the auditory threshold is on the whole unnecessary, and is in some cases actually detrimental when a practised listener is being tested;

(*h*) that conditions of diffuse illumination tend to induce in the listener an attitude which is judged to be favourable, but that in the early stages of learning to recognize a sound, darkness provides the more favourable condition;

(*i*) that the responses to imagined sounds (1) occur in groups; (2) are most frequent during the initial period, before attunement, and at a later period when fatigue may be operative; (3) in the later periods of a spell of listening, as compared with the earlier, show a decrease in number together with an increase in the omission to react to presented sounds;

(*j*) that there is very little evidence of sensory fatigue, and that such improvement as may be effected appears to be due chiefly to growing familiarity with the sound presented, and with the general conditions of listening.

An attempt to make a qualitative analysis of the process of listening to sounds of weak intensity shows:

(i) that a series of presented sounds tends naturally to be classified on the basis of differences noted by the listener, and that such classification appears to be effected for the sake of that identification of sounds which plays an extremely important part in hearing;

(ii) that presented sounds are frequently, but not invariably, localised, being accorded position, and distance; and in some cases movement also may be attributed;

(iii) that sound stimuli may often be perceived when they cannot be heard as sounds;

(iv) that the cessation of the physical stimulus frequently gives rise to a positive experience of silence;

(v) that subjective sensations of extreme vividness often occur, which however may to some extent be discriminated from sounds having an objective basis;

(vi) that many other factors besides the purely auditory and sensory help to determine reaction to sounds of weak intensity;

(vii) that external distracting sounds are most disconcerting when they are irregular, or like the test sound, or of a familiar character;

(viii) that on the whole a subject's judgment concerning the efficiency of his reactions is likely to be accurate only when that judgment is a favourable one.

(*Manuscript received* 5 *August*, 1919.)

PSYCHOLOGY AND EDUCATION[1].

BY T. P. NUNN.

THE meeting over which I have the honour to preside marks an occasion of great moment in the history of the British Psychological Society. The Society has recently decided, after due deliberation, to depart in two important respects from the policy which has hitherto guided it. On the one hand, its membership will no longer be confined to professional students and teachers of psychology, but is to be thrown open to all persons whose interest in the science is suitably attested. On the other hand, following the Spencerian law of progress, it proposes that a large part of its work shall henceforward be carried on by autonomous sections, each concerned with a special department of psychological inquiry. The first of these two departures is clearly justified by the immense increase in prestige that has accrued to our studies during the war. The psychologists have been mobilised side by side with their fellow men of science, and have shown that they, too, had invaluable service to give to the nation in its hour of need. We owe it mainly to these men—of whom we are proud to claim a large proportion, and among them by no means the less eminent names, as members of our small Society—that the thinking public has become deeply impressed by the great practical value, actual and potential, of exact psychological inquiries. Without doubt, then, the time has arrived when our Society may safely come out from its academic seclusion, and invite the adhesion of all who recognise that in the field of modern industry, as well as in the territories occupied by the ancient professions of medicine and teaching, there are problems, of great moment to human efficiency and happiness, that cannot be solved except with the aid of psychological knowledge and psychological methods. The second new departure—the institution of special sections within the Society—follows almost necessarily from the first. I need, perhaps, say no more about it than to remind you that the Spencerian formula, to which I have already referred, specifies progressive integration as well as progressive differentiation as the law of evolutionary advance. In other words, we must take steps to secure that the several Sections, in performing their distinct functions, always work as organs of a single Society informed by the true spirit of scientific psychology, which is, and for ever must be, one and indivisible.

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[1] An Inaugural Address, here in abbreviated form, to the first Meeting of the Educational Section of the British Psychological Society, 11 April, 1919.

It happens that ours, the Educational Section, is the first of these organs to be constituted, and to-day begins its formal existence. In what is largely an accident of fortune, we may yet modestly note a certain appropriateness. Not only is it true that the researches of some of the most valued members of the Society have been, particularly in recent years, important contributions at once to psychology and to education; it is also true that, of the wider fields of work which the new Sections are intended to cultivate, ours is, perhaps, the one least choked with the weeds of ignorance and prejudice, and has, in fact, already been ploughed for the sower. A few years ago, one of my colleagues advised a student not to use coloured chalks to illustrate his lessons in a secondary school, for fear the headmaster should suspect him of introducing psychology. The witticism would have little point to-day. Most teachers have travelled far beyond the point represented by the discovery which startled the honest Monsieur Jourdain. They not only recognise that, without knowing it, they were always psychologists; they are anxious that their psychology shall have the sanction of accredited authority. Indeed, it may be said that there is now, in some quarters of the educational world, an exaggerated estimate of the value of psychology: a disposition to suppose that laboratory experiments and the measurement of correlations can and should supply the whole basis of educational procedure.

To think thus is, in my judgment, to misconceive the true relation between psychology and education. To make progress, education must, no doubt, accept increasingly scientific regulation; but it can never be reduced, any more than life itself, to an applied science. Education is a biological function more ancient even than man, for it is found in a rudimentary form among the animals. Whatever we do or abstain from doing, the basal conditions of human life will ensure its permanence as a tradition of social activity, and will determine its general form. In relation to that great vital function, the psychologist must always be contented with the position of a critic, whose primary business is not to determine the aims of education, but to secure efficiency and economy in the means by which those aims are pursued. He is concerned with the aims of education only in a secondary way, in as far as his criticism of traditional procedure may lead to what is, in effect, a revaluation of accepted ideals.

This standpoint may be illustrated by a couple of simple instances. The art of writing is not an invention of the psychologist, and he is certainly not to be held responsible for the irrational spelling which the English-speaking peoples have inflicted on themselves. As things are, the psychologist must, like the schoolmaster, accept the binding of this ridiculous burden upon the young as one of the minor ends of education. His immediate task is, by the analysis of the writing-process, to make its acquirement as economical and effective as possible. Yet in performing this task, in criticizing from the psychological standpoint the accepted procedure, he paves the way to a reconsideration of issues much wider than the one originally presented to him. Thus not only do both advocates and opponents of spelling reform appeal to psychology—as, for example, Dr Henry Bradley, a moderate opponent of 'simplified spelling,' has done in his recently reprinted article on 'Spoken and Written English,'—but also many teachers have been led, by the present concentration of interest upon this question, to revise their estimate of the place of the art of writing in elementary education, and to value it as contributory to another art—in the aesthetic sense—namely, the sadly neglected art of clear and beautiful expression in speech.

In this instance, then, the direction of psychological analysis upon a problem of ways and means has, concurrently with other influences, led to a revaluation of the aim of the process. Similarly, the study of the technique of memorising, begun long ago by Ebbinghaus, has not only led to improvements in the practice of the schools, but also, through a fuller appreciation of the difference between 'mechanical association' and 'memory of meaning,' has contributed to an important change of view about the place and value of memorising in teaching and study.

The protracted controversy about 'formal training'—a question which was made a burning issue in this country by the brilliant criticism of Professor John Adams, and has been the subject of important experimental researches by two of our younger members, Mr W. H. Winch[1] and Dr W. G. Sleight[2]—illustrates my principle still more aptly. For the accumulating disbelief in the transfer of acquired ability from its original field to another has had a most pronounced effect upon educational thought. On the one hand, it has led, we may believe, to much sounder ideas about the nature of the 'mental training' produced by school studies; on the other hand, by freeing the minds of educationists from bondage to an ancient fallacy, it has encouraged them to look for, and, I think, to find, a much better theory of the curriculum than was formerly current.

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[1] Cf. This *Journal*, II. 1908, 284–293; III. 1910, 386–405.

[2] Cf. This *Journal*, IV. 1911, 386–457.

The psycho-physical mechanism of writing, the laws of memorising, the transfer of acquired ability are all topics whose educational relevance is immediate and obvious; their educational relevance is, in fact, the main source of the psychologist's interest in them. This cannot be said of certain other questions around which psychological debate has largely centred during recent years. For example, the theory of education cannot claim proprietary rights in the ideas of Dr Wm. McDougall and Mr A. F. Shand on the nature of instinct and the psychology of the emotions—ideas that were, I believe, first unfolded before this Society. Nevertheless their quickening influence, which has affected all the sciences that draw from psychology, has nowhere been felt more strongly than in education. The reason is manifest. McDougall's theory of instinct, supplemented by Shand's doctrine of sentiments, has thrown a flood of new light upon the genesis and laws of development of human behaviour; it is not surprising, therefore, that the writings of these psychologists, though they make only scanty references to educational problems, are among the main pillars of modern pedagogy.

It is my belief that the same thing will ere long be as evidently true of the ideas we owe to the profound insight and patient labours of Professor S. Freud and Dr Carl Jung. You may think that, in coupling the names of these two men together I show an imperfect appreciation of the divergence between their views. I am aware that the divergence is as wide as it well can be—being, in fact, the expression, in psychology, of the difference between the 'mechanistic' and the 'vitalist' conceptions of life. Experience teaches us, however, that differences even so profound as this, may yet be composed. We have a happy omen of future reconciliation in the attitude of physiologists like Dr J. S. Haldane, whose firm conviction of the teleological character of such a process as respiration has not prevented him from making additions of signal value to our knowledge of the mechanism involved in it.

This is not the place to develop the belief I have just acknowledged, nor to discuss the features of the Freudian doctrine which prejudice it in the eyes of many educationists. I may, however, be permitted to say that, from the standpoint of education, it was unfortunate—though no doubt inevitable—that the doctrine was developed, primarily, to explain pathological phenomena. To this circumstance we owe, for instance, the fact that the term 'complex' carries, by definition, a connotation of morbidity, although there is, from the strictly psychological point of view, no difference between the activity of morbid complexes and the structures that underlie most of our normal behaviour. It is in my view unfortunate that a term so aptly fitted to describe a general and fundamental feature of mental life should, by the accident of early usage, be restricted to morbid phenomena. It is due to the same cause that the concept of 'repression' occupies what I venture to think is a disproportionately large place in the new psychological scheme. Undoubtedly the facts of repression have immense importance for teachers as well as for psychiatrists. Yet to think of 'the Unconscious' as containing nothing but repressed material is, I believe, to miss much of the biological significance of the idea. In my view, the chief value of the concept is to remind us that conscious activities are only one—though the highest—of the means by which the psycho-physical organism conducts that intercourse with the environment by which and in which it lives. Consciousness marks the growing-point of our higher activities, the edge by which they 'cut into reality.' Behind this point, this edge, there is a vast impulsive organization of which a great part is, I believe, never represented directly in consciousness, while, of the residue, much that has once been conscious can never normally, and in its own character, reach the conscious level again. Nevertheless, the movements of consciousness, subserving the organism's perpetual self-assertion in the face of its environment, are never wholly explicable apart from this organization, whose history and constitution they express in an endless variety of subtle ways.

The central feature both of McDougall's theory of instinct and of Freud's doctrine of the unconscious is the insistence upon the importance of the affective elements in human life. Parallel with these researches, there has been another remarkable development of psychological inquiry in a field which is more obviously educational. I refer to the inquiries into the nature of ability, and the modes of testing it, which have been the subject of so much patient and acute investigation, and threaten to convert psychology, to the discomfort of many, into a branch of applied mathematics. In this connexion the name of Alfred Binet will always be honourably remembered, and his attempt to establish a 'metric scale' of intelligence opened a chapter of experimental pedagogy that is as yet by no means closed. Binet was concerned, as you well know, with a practical problem which forces itself upon the attention of educational administrators in every great city—namely, the problem of determining whether the backwardness that so often makes it impossible for a child to keep pace with his coevals is, in a given instance, due to mental defect or merely to unfavourable conditions, such as constant removal from school to school. I will say nothing about this question except to remark upon the admirable way in which Binet's original work has been continued by Mr Cyril Burt, whose recent survey of educational abilities in a London borough is a shining example of the value of the new statistical methods.

For most of us there is matter of greater interest in wider inquiries that have, in part or wholly, grown out of Binet's. As Professor Spearman has pointed out, Binet tacitly assumed a certain view as to the nature of the abilities manifested in the various types of intellectual performance —namely, that they are, in the main, only different expressions of a single factor whose value is, so to speak, a coefficient which varies from person to person but is constant for a given individual. To the theoretical question here raised, the question whether abilities are 'non-focal,' 'multi-focal' or 'uni-focal,' Professor Spearman's own contributions are of unique and outstanding importance. He has defended his acceptance of the 'uni-focal' view in a series of memoirs that have attracted widespread attention, and have given rise to a vast amount of most useful discussion. It is true that the validity of the argument upon which he chiefly relies—an argument which is itself of the highest novelty and interest—has recently been called acutely in question by Dr G. H. Thomson[1]; but my faith is that Professor Spearman has a satisfactory reply even to so ingenious and penetrating a critic. At the same time, I think it possible that—as has so often happened in parallel instances in the history of science—his simple conception of the 'central intellective factor' may have to be qualified or elaborated.

Meanwhile Spearman's characteristic method has been applied, with results which are, to say the least, both interesting and promising, to the further exploration of the fundamental factors in mental activity. First we had Dr E. Webb's important and excellent research[2] into the psychological elements that determine the judgments men pass upon the character of their fellows, and his discovery that these judgments imply the presence of a second 'central' factor which he has called, provisionally, the 'persistence of motives.' And this year we have had from Mr J. C. Maxwell Garnett[3] the announcement of a third elementary factor, 'cleverness,' together with the fascinating suggestion that the three factors signalised by Spearman, Webb and himself may be regarded as co-ordinates fixing the general character of an individual's endowment. While the field opened up by these later developments from Spearman's researches is, admittedly, one that demands further exploration, there can be no question of its great interest both for general psychological theory and for education.

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[1] This *Journal*, IX. 1919, 321–336.

[2] This *Journal, Monogr. Suppl.* 1915, I. No. 3.

[3] This *Journal*, IX. 1919, 345–366.

My faith in the essential soundness of Spearman's doctrine of the central intellective factor is confirmed by the growing evidence of the practical efficiency of 'intelligence tests'; for it seems difficult to explain this efficiency on any other basis. By far the most impressive evidence of this kind is that afforded by the grandiose experiment in the American Army, in which about a million and a half recruits were submitted to tests designed to pick out the men of superior ability, to eliminate those whose inferiority would make them a source of hindrance or danger to their fellows, and to enable the authorities to equalise the intellectual strength of military units which were intended to work together. The comparison, in some thousands of instances, of the results of a fifty minutes' test with the carefully considered judgments of officers about the men's efficiency in the vastly different circumstances of the camp and the training school, is extraordinarily favourable to the value of the tests. It is not surprising that at least one American University is said to be adopting intelligence tests as a supplementary means of discriminating between the claims of its would-be alumni, and that in this slower-moving country the use of such tests as a means of grading County Council scholars and other entrants to secondary schools has received a very great impetus. Here is indicated a field in which our Section may find extremely useful work to do; for if this outburst of faith in psychology is not cautiously and capably guided, it may end in disillusion, and in bringing the use of intelligence tests into contempt.

The same thing is true of the growing belief in the value of 'vocational tests,' which may be regarded, speaking broadly, as related to Spearman's 'specific factors' in the same way as intelligence tests are related to his central factor. At least one local educational authority is reported to be willing to pay for expert psychological opinion about the capabilities of the young people in its schools. The movement thus initiated offers scope for much mischief as well as for good work. Teacher-members of the Section who have had some psychological training could hardly do more useful service than by taking in hand the cautious and systematic study of individual pupils from the standpoint suggested by the concept of vocational tests.

In an hour's space it has been possible to glance at a few only of the more important departments of educational psychology, and that only in the most cursory way. It is evident that in these, as in many other directions, a great deal remains to be done in order to perfect the control of our professional activities by scientific regulation. It is gratifying to our pride to observe how much of what has been done is to be credited to members of the British Psychological Society. The Educational Section has been constituted to widen and deepen this good and necessary work. May success attend its labours!

PSYCHOLOGY AND INDUSTRY[1].

BY CHARLES S. MYERS.

PSYCHOLOGY must be ranked to-day among the Natural Sciences, standing in the same relation to the living Mind as Biology stands to living Matter. Biology, as is well known, has been served by two different methods of approach. There is the older 'observational' method, which has been employed in such general problems as natural selection, variation, adaptation, etc., and in compiling the Natural History of living organisms—their development, their generic, specific, individual and sexual differences, etc. And there is the 'experimental' method, which, in conjunction with the former method, has given rise to physiology, pathology, genetics, etc.; these have rapidly advanced to the position of Applied Sciences in relation to medicine, human and veterinary, to eugenics, animal and vegetable, etc.

In its earliest days Biology was a field of study for philosophers, who employed both the observational and the experimental methods of approach. Psychology, on the other hand, is only just emancipating itself from the tutelage of Philosophy. In Psychology we can likewise distinguish the observational method, which has helped to reveal to us the Natural History of mind, from the later experimental methods which have been characterized by a more thorough knowledge and a more perfect control of attendant conditions, and by a more complete freedom from metaphysical preconceptions and aims. The effects produced by varying conditions upon mental experience (or introspection) and upon bodily expression (or behaviour) have come to be studied in the laboratory, where those conditions can be simplified or complicated at will. Advantage has also been taken of Nature's own variation of such conditions, as in the clinical and laboratory study of *individual* mental differences, normal and abnormal, of excess or defect, including those produced by disorder, disease or injury, *racial* mental differences, *e.g.* the mental differences between the higher and lower races, and *generic* mental differences, *e.g.* the mental differences between the sensations, intelligence, instincts, etc. of man and animals, of vertebrates and insects, and so on.

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[1] The substance of an Inaugural Address to the first Meeting of the Industrial Section of the British Psychological Society, 25 April, 1919.

Still more recently another stage in the evolution of Psychology has been reached by the systematic study of unconscious processes and of their relations to consciousness. Whereas the earlier philosophical psychology, and the experimental school which arose from it, had been mainly intellectualistic, giving undue prominence to the play of reason, this later stage has been characterized by the emphasis it lays on the importance of instinct and the emotions, and by its devotion to the study of unconscious processes.

As in the case of Biology, the results obtained from experimental psychological methods, and indeed those methods themselves, have begun to be applied to practical purposes—first to Education, next to Medicine, and most recently to Industry, thus creating three Applied Sciences, those of Educational, Medical and Industrial Psychology; and the British Psychological Society is now instituting three Special Sections of the Society which are to be respectively devoted thereto.

In Industry (including Commerce) there are four main themes to which Psychology can be profitably applied, namely to fatigue, movement study, vocational guidance and management.

Fatigue has long been a subject of research both by physiologists and by psychologists. The physiologist has generally investigated it under the simplest experimental conditions. For example, he has isolated a single muscle of a frog from the body, and has studied the phenomena of fatigue produced in it by electrical stimulation of the muscle or of its nerve, and the effects of varying the strength and frequency of stimulation, the surrounding temperature, the weight lifted by the muscle, etc. He has also investigated the effects of muscular exercise on the general metabolism (the respiratory quotient, the excreta, etc.) of the organism. The psychologist, on the other hand, has conducted exclusively 'human' experiments, treating the organism as a whole in place of using 'muscle nerve' preparations. He has approached the problem from the standpoint of mental as well as muscular fatigue. He has devised tests of mental fatigue, constructing work curves of mental output, and analysing the psychological factors involved therein, such as spurt and practice, in turn analysing the latter into its psychological components. He has studied the effects of drugs, *e.g.* of tea, coffee, strychnine, and alcohol, on mental and muscular fatigue. He has examined the effects of rest pauses of different length, introduced after varying periods of work, on mental efficiency. He has shown the unreliability of certain interpolated tests as evidence of muscular or mental fatigue; he has shown the importance of a rigorous, precise training in the methods of experimental psychology, in order to avoid the pitfalls incidental to human experiment; and he has so prepared the way for a systematic investigation of the more important problems of industrial fatigue that future success must depend on intimate psychological and physiological coöperation.

Movement study has as yet been scarcely touched by the psychologist. It has hitherto been mainly the purview of the industrial 'efficiency expert.' But there is obviously a vast field of promising psychological research here. The present methods are largely empirical and guess-work. The expert pays a visit to a factory where he sees a worker making a series of seemingly needless movements. He believes that time would be saved by training the worker to another, an apparently 'shorthand,' method. He tries it and, we will suppose, he finds that time is saved by its adoption. He assumes that, because a speedier method has been devised, there is no increase, or there is a decrease, of fatigue. He assumes that because this method is found to suit one worker, it is therefore the one and best method, always to be adopted by all workers. Clearly there are numerous problems for psychological experiment here, by which the applied science of Industrial Psychology will be advanced to surer ground. Similar scientific work is needed to yield reliable information in regard to other matters which are intimately connected with movement study, *e.g.* the optimal load and posture, the optimal rate and frequency of lift, etc., in persons of different muscular power, age and sex.

The study of vocational guidance is founded on that of individual differences, for the basis of which we are indebted to pure experimental psychology. Some of the earliest psychological experiments on reaction time were devoted to a study of the individual differences observed. The advantages of selecting employees in the case of certain occupations according to their speed of reaction have been shown in a certain bicycle-ball factory, where, after the selection of the best workers as indicated by reaction tests, it was found that 35 individuals could now do the work of the previous 120, and that the accuracy of the work was increased by two-thirds.

Similar results have followed from the application of psychological tests to the selection of applicants for telephone-exchange work in America. It is obvious that their hearing, vision, speech, memory for figures, memory for the order of instructions received, and their speed and accuracy of reaction to signals are readily capable of experimental estimation.

It is easy to devise tests of manual dexterity and these are already being employed in America in the selection of employees for work in which such dexterity is important.

Psychological tests of foresight are also capable of construction, and have actually been applied with success in investigations on motor tram-drivers. A very close inverse relation was found to obtain between the degree of a driver's success in the laboratory test and the number of accidents recorded against him during his every-day work.

Tests of sensory acuity and discrimination, tests of artistic endowment, tests which measure fatigability, accuracy, neatness, distractibility, improvability, memory for names and faces, powers of observation, etc. are also available. Their application to those who offer themselves for designing work, clerical work, salesmanship, etc., is obvious.

Tests of the accuracy and speed of reasoning have also been devised. Tests of general information have been frequently employed. These and other tests are about to be used at Columbia University, New York, in place of the Matriculation Examinations, so as to select those who can best profit by a University career.

During the last year of the war, I happened to be concerned in the selection, at the Crystal Palace, of candidates for training in hydrophone-listening for hostile submarines. Tests were devised for keenness of hearing, accuracy of sound discrimination, memory for pitch, rhythm and quality of sound, power to discriminate between different pitches, rhythms and qualities, general accuracy, general information, ability to grasp complicated instructions, etc. The result of the application of such tests was that the training authorities at Portland reported that the first batch of lads sent them from the Crystal Palace was far away the best they had ever received, and that the next batch was even better still!

It is perfectly clear that by the aid of properly devised tests applied by properly trained persons those leaving school could be materially helped and usefully advised in their choice of a suitable vocation, and that by their application to candidates for any industrial or commercial post the fittest could be speedily selected.

An objection may be raised that these tests throw no light on the higher moral qualities of the candidate, such as perseverance, resourcefulness, good temper, loyalty, honesty, courage, self-control and 'presence.' But in point of fact, the presence of the first two of these qualities are revealed in many existing tests or in others that can be specially devised for the purpose, whilst much light is (or can readily be) thrown on the rest in the course of individual examination and cross-questioning.

None but those who have had experience in psychological tests can realise what a wealth of information in regard to the 'character' of a subject is incidentally gained from tests systematically and *individually* applied: although the 'lower' mental characters can be easily tested in groups of fifty or a hundred or more persons simultaneously.

Under the application of Psychology to management, I include the consideration of the psychological causes of industrial discontent and restricted output, the psychological advantages of different methods of payment and supervision, and other conditions which affect the efficiency and the happiness of the workers. During the last few years a flood of light has been thrown on the importance of the emotions and on the changes which they effect and to which they are subject. We now recognise how prone we are to rationalise, *i.e.* to give an intellectual reason for actions which are really prompted by emotional states, or by subtler influences which are unknown to us or which for good reasons dare not be faced. We now recognise that in order to avoid causing excessive self-depreciation an emotion may undergo a process of 'projection.' Thus instead of reproaching ourselves we may attribute the reproach to others; hence arise delusions of suspicion and even persecution. Or, for the same purpose, an emotion may be 'inverted,' *e.g.* shyness becoming concealed by an affected boisterousness, the desire for a person of the opposite sex by aversion, submissiveness by defiance. We understand now more fully the psychological basis of worry and anxiety, the importance of their early treatment, and the psychotherapy of the functional nervous disorders to which, if unresolved, they may give rise. The application of such new advances to the problems of industrial unrest is sufficiently obvious.

It may be asked whether a Special Section of the British Psychological Society is the most appropriate body for the promotion and communication of researches bearing on Industrial Psychology, or whether a separate society should not have been constituted in its stead for these objects. To this question, I think, there is a very clear answer. The British Psychological Society is henceforth to consist of three Special Sections devoted to Education, Industry and Medicine, and of a General Section devoted to the other aspects of Psychology, from which it is hoped in the near future to form further Sections for Social Psychology, the Psychology of Æsthetics, Animal Psychology, etc. The Society consists, therefore, of members interested in every branch of knowledge to which Psychology is applicable. It will afford opportunity for the holding of joint meetings between Sections, at which subjects of common interest can be profitably discussed. Thus, the Industrial and Medical Sections could advantageously combine in considering the psychology and psychotherapy of industrial neurasthenia: the Industrial and Educational Sections could profitably hold a joint meeting to discuss from the psychological standpoint the best arrangements for industrial education. Each Section of Applied Psychology must thus reap advantages by intercourse with the other Sections, while the presence and coöperation of those engaged in pure psychological research cannot fail to lift Applied Science to a higher plane.

PSYCHOLOGY AND MEDICINE[1].

BY W. H. R. RIVERS.

WE are met this evening to inaugurate the foundation of a Special Section of a Society which has hitherto attempted to cover unaided the whole field of psychological inquiry. The great increase of interest in, and knowledge of, the mental aspect of disease which has been one result of the abnormal strains to which modern warfare has exposed the soldier made it certain that something would be done to foster this interest and increase this knowledge. On the more practical side, and in its relation to medicine in general, the medical profession is already provided with instruments for this purpose in the Psychiatrical Section of the Royal Society of Medicine and the Medico-Psychological Association; but the work of these bodies is chiefly connected with the practical aspect of medicine. It is not their business to attend in any special measure to the theoretical aspect of psychology. Still less is it their function to deal with the relations of their work to other branches of psychology.

As soon as the declaration of the armistice became effective, there arose a widespread opinion that some organization was necessary to encourage the more theoretical side of psychological medicine. It became a question whether this purpose would be fulfilled more effectively by a society wholly devoted to this purpose, or whether the new organization should become part of the society which has now for many years been the meeting place of the psychologists of this country. This question has been decided in the second sense, and on this occasion, when the Medical Section of our Society meets for the first time, I cannot better employ the privilege you have given me of opening its scientific work than by pointing out some reasons which justify this decision.

I will begin with a general problem. One of the most vexed questions of to-day is concerned with the good and the evil of specialism. With the great advance in knowledge of which we are now enjoying the fruit, specialism is necessary. Owing to the vast extent of the field it is essential that workers in medical as in other branches of science shall not be content with a general knowledge of the subject to which they devote their labours, but shall attend specially to some one of the many aspects which every branch of knowledge now presents. This specialisation has, however, in recent years reached such a pitch that it has become a serious evil. There is even a tendency to regard with suspicion one who betrays the possession of knowledge or attainments outside a narrow circle of interests. Scientific workers often deliberately confine their research to some narrow channel. They fail to see the bearings of work (including their own) which would be obvious if they lifted their heads and surveyed even cursorily the broad field of knowledge of which their own specialty forms one of the fertilising streams. The linking of workers in psychological medicine with other students as members of a society which covers the whole field of psychology should go far to prevent the evil of undue narrowness of outlook and limitation of interest.

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[1] The Inaugural Address to the first Meeting of the Medical Section of the British Psychological Society, 15 May, 1919.

It is proposed that the Society to which we now belong shall have several Sections, but owing to the fewness of their adherents students of certain subjects well adapted for Special Sections will for the present have to be content with the opportunities offered by general membership. I propose now to survey briefly some of these branches of psychology and to inquire how we may expect to benefit from association with those who study them.

I will begin with the parent, an aged parent, whose services in the past we are perhaps inclined to depreciate unduly. The older Introspective Psychology which came into existence by a long-drawn-out process of fission from philosophy, with which it still struggles to maintain its connexion, did great service in defining the problems of psychology and analysing into its elements the vast complexity of mental process. Into this process of analysis it brought from its earlier associations a clearness and exactness of thought which may well be emulated by the adherents of the later developments of psychology. The great fault of the older psychologists was that their liking for clearness of reasoning and exactness of definition led them to pay a far too exclusive attention to the intellectual aspect of mind, where exactness is possible. It led them to ignore, or to pay too little attention to, the subjects of instinct, feeling, and emotion, which are less susceptible of exact treatment. They also tended to neglect unduly mental process lying on the confines, or altogether without the confines, of the conscious. The adherents of the older academic psychology are now aware of the imperfections of their earlier position. But they have not lost the logical minds which they acquired from their special training, and there is the widest scope for mutual advantage if they will take part in our discussions and if we attempt to understand the point of view from which they regard our special problems.

Next in order, I may take the Experimental Movement in psychology. There is now a widespread, and in my belief a well-founded, opinion that this movement has failed to come up to the expectations of its founders and has proved unfruitful as a direct means of increasing our knowledge, at any rate in so far as it confines its attention to the experimental investigation of the normal human adult. It cannot be said to have done much more than provide suggestions and clues for investigation on other lines. It is not difficult to see the reason for this. In the case of the normal human adult, there is too little scope for the variation of conditions which is the essence of experiment. It is only such subjects as are open to the definite variation of conditions that have provided material for any great advance in knowledge.

While the experimental method as applied to the normal adult has borne little fruit, it would be difficult to rate too highly the importance of experiment in discovering and testing methods to be used in other lines of psychological inquiry where a wider variation of conditions is possible. The value of these methods is now generally recognised in the investigation of the developing mind of the child and of the mental processes of animals. These methods are also essential to the new department of industrial psychology, but there is no branch in which they are more needed than in psychological medicine. Thus, the clinical work of Head and his colleagues, which must take a foremost place in the history of psychology, has as one of its main features the utilisation of the methods of experimental psychology modified to meet the special needs presented by clinical material. Experimental psychology takes a place, in relation to the observation of data derived from behaviour, as important as that which is taken by the older introspective psychology, in relation to the logical processes by which these data are utilised.

It is not at present proposed that the two branches of psychology which I have just considered shall have Special Sections devoted to them. Members of this Section should be able to gain such help as they require by the attendance of general and experimental psychologists at the meetings of this Section, while they may also learn much from the papers and demonstrations given at general meetings of the Society.

The subject of Educational Psychology, for the study of which a Section has already been formed, is one from which members of this Section should have much to learn. It is now widely agreed that much of the general psychological state which predisposes a person to the occurrence of neurosis, and to a less extent of psychosis; comes from faults of education, using this term in a wide sense to include the whole life-history of the child from its birth. Opinions differ concerning the relative weight of these factors as compared with heredity in the production of the neurotic and psychotic constitution, but no one can use modern methods in the clinical investigation of psycho-neurosis without having forced upon him the vast importance of the mental traumata and faulty trends of thought and conduct which occur or come into existence in childhood, often in its very early years. Nothing is more needed to help advance in our special field of knowledge than close collaboration between those who deal with sufferers from the psychoneuroses and those who have the care of children during the years when the seeds of these morbid states are sown. May we hope that joint gatherings, mutual attendance at the meetings of both Sections, and the general opportunities for interchange of views which this society should provide, may furnish material for rapid progress on lines which will bring great mutual benefit to both physician and teacher.

The other Section, which has been already inaugurated, will deal with Industrial Psychology. Within its scope will come the investigation of fatigue, interest, practice, and other mental factors which affect the efficiency of labour, both manual and mental. It will also deal with the mental qualities which fit or unfit a person for an occupation, and thus help in the highly important task of putting "round pegs in round holes and square in square." Investigations of this kind will not be limited merely to the needs of government departments or to the demands of industry, but will, doubtless, be extended to such subjects as the choice of a profession, and will thus closely approach the work of the Educational Section. Here, perhaps, the psychologist of industry will have to learn from the physician, and members of this Section will, perhaps, visit the Section of Industrial Psychology to teach rather than to learn, but it is very unlikely that the advantage will be altogether one-sided. Nothing but good can come from common membership of this society with its opportunities of collaboration.

The two Sections to which I have just referred will resemble our own in that they will be devoted to applied psychology. It is not at present proposed to found a Section for the branch of psychology which I have now to consider, though this cannot be long delayed. The science which deals with the behaviour of human beings when acting as members of a group, and not merely as individuals, is usually known as Social

Psychology, though it might also be called Collective Psychology. It attempts to explain the actions of men as members of a social group, whether nation, tribe, clan, church, profession, club, or any other kind of combination in which men unite for social purposes. Psychology is here removed from the possibility of utilising experiment as a means of eliciting truth and has therefore had recourse to the comparative method. Social psychology is largely devoted to the behaviour of human society in its simpler and cruder forms, whether of the past or of the savage and barbarous peoples who still occupy the less comfortable regions of the earth. Through the study of the culture of these peoples it is becoming possible to form some idea of the mental factors which have determined the course of human evolution and the outcome of the interactions, peaceful or otherwise, which come into being when peoples are brought into contact with one another.

It might seem at first sight that we have here a branch of psychology which can have little in common with the work of this Section. The knowledge gained in this new study, however, has already brought out some striking parallels between the behaviour of the ruder forms of human society and the thoughts and actions of civilised man under such abnormal conditions as come within the purview of this Section.

The outstanding fact which is daily becoming clearer to those who are studying the various forms of mental disorder is that these depend upon the re-entrance into activity of instinctive tendencies which have been brought under control in the normal healthy person, and have become subject to standards of thought and conduct in harmony with the needs of social life. In man many instinctive tendencies subserving the welfare and immediate happiness of the individual have been suppressed because they are incompatible with the needs of society, and with the ultimate happiness of the individual as a member of society. Neurosis and psychosis are essentially states in which the controlling and suppressing forces have been so weakened that conflicts are aroused in which the suppressed tendencies strive with more or less success to regain the predominance they once held, both in the history of the race and in the infancy of the individual.

The frequency of the psycho-neuroses in the great communities of the modern world is the direct consequence of the fluid and unorganized character of their civilisation. In savage communities where long ages of freedom from external influence have allowed culture to become organized and stable, the psycho-neuroses are absent or hardly to be detected, although the cruder forms of mental disorder, such as imbecility and idiocy, are not uncommon. In such communities there has come about a stable adjustment between instinctive tendencies and social ideals, leaving no room for the conflict which forms the essence of the psycho-neuroses. Similarly, I believe that a comparative study of the frequency and severity of neuroses among the great civilisations of the modern world, will show that this frequency and severity are definitely correlated with the fluidity or instability of the culture and with the extent to which national ideals call for repression of instinctive tendencies. The perfect social organization is one in which instinctive tendencies, out of harmony with social ideals, have so come under control that they no longer form the grounds of conflict or give occasion for it only in the presence of exceptional stress and strain.

Another interest of social psychology to the student of mental medicine, is that when men act collectively they tend to be moved predominantly by motives belonging to the instinctive sphere. Not only does each individual of the mass act more instinctively as a member of a group than as an individual, but when a number of men act together, the rational motives arising out of education, and other forms of social process, tend to cancel one other, leaving in power the instinctive tendencies which are common to all and of the same nature in all. It is this potency of instinct which is common to the behaviour of men in mass and the individual in disease which gives a common interest to the physician and the social psychologist. It furnishes a reason in itself sufficient to justify the inclusion of both in one organization for the advancement of knowledge.

Of even more interest to the physician should be the study of mental process in rude and backward forms of human society. Much as we may disagree in detail, there is general agreement that in neurosis and psychosis there is in action a process of regression to primitive and infantile states. Anything which helps the physician to a knowledge of the primitive and infantile in man should therefore come within his circle of interests. In so far as the thought and behaviour of savage man are primitive, they furnish material which helps us to understand and to deal with the regressive states exhibited by sufferers from disorder of mental functions.

From the point of view of the ethnologist the problem is far from simple. The customs and institutions of existing examples of savage man show such a bewildering mixture of development and degeneration that it is difficult to disentangle the primitive from the late. If it stood alone ethnology would be greatly embarrassed by the complexity of its problems, and it is driven to seek help in the study of other aspects of human behaviour. If regression is a constant feature of morbid mental states, and of such a normal process as the dream, it should furnish much material to help the seeker after the primitive in human conduct. In a recent publication[1] I have tried to show how the psychology of the dream may help us to determine the nature of the primitive, and quite as much is to be learnt from the psychology of neurosis, which, as we are coming to see, has so much in common with the dream. Medicine standing alone and ethnology standing alone may find themselves helpless before problems the solution of which will come from the union of the two lines of inquiry, at first sight so widely different from one another. It is difficult at present to say exactly how each can help the other, but the general trend of our knowledge justifies the brightest hopes if students of each will work together in such a spirit of coöperation as it is the object of this Society to foster.

Another branch of psychology is one which might at first sight seem even more remote from the interests of the physician than that I have just considered. The study of the mental processes of animals is one in which this country has had, and still has, great names, but the number of its votaries is at present so small that there does not seem to be any immediate prospect of the foundation of a Special Section devoted to this study. Work on this subject will for the present form part of the business of the general Society, but however it enter into the scheme of our activities, Animal Psychology should have a profound interest for the physician, and for reasons similar to those which justify his interest in the ruder forms of human culture. If instinct have the importance which we are, I think, all willing to recognise, animal psychology, from which issues our chief knowledge of instinct, cannot be neglected by this Section. At the present time there is much confusion and uncertainty in the use of instinct as a psychological concept. In clearing up this confusion we need the coöperation of those whose studies lead them to an interest in the primitive, whether it be exhibited by the animal, the child, the savage, or the subject of regression. We do not know from which of these will come the clue leading us to the truth, but my impression is that morbid psychology has the brightest prospects, and that this subject is destined to illuminate many of the dark regions in our knowledge of mental development. Here, again, the opportunities of collaboration offered by this Society should furnish an ample reason for our union with other branches of psychology.

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[1] W. H. R. Rivers, *Dreams and Primitive Culture*, Manchester, 1918.

Thus far I have been considering the work of this Section in its relation to the general aims of the British Psychological Society and to the work of its other Sections. It is now fitting that I should say something about the special tasks which lie before our own Section. The most obvious task is to understand the functional nervous and mental disorders, and to make them instruments by which we may be helped to understand the normal working of the mind. Most of us are engaged, or have been engaged during the war, in the practical task of treating sufferers from disorders which, notwithstanding the apparently physical character of many of their manifestations, are now generally recognised as being wholly or in the main of mental origin. It is our special task as members of this Section to study these disorders in order that we may carry out that process of bringing them into relation with other fields of knowledge which we call 'explanation.'

Many of us have been brought into contact with the psycho-neuroses only through the accidents of war, and a word or two may therefore be said about the special interest of war neurosis from the psychological point of view. In my opinion, the keynote of the war neuroses is their simplicity. They are primarily due to the reawakening of suppressed instinctive tendencies which, in most members of our civilisation, are normally allowed to lie dormant because the mechanisms by which they are controlled and suppressed are subjected to no strain great or continuous enough to interfere with their efficiency. Moreover, the instinct which is mainly affected, the instinct of self-preservation, is one of great simplicity, while the social and intellectual elements which form factors in the process of control are also of a relatively simple kind. Through this simplicity the psycho-neuroses of war are well adapted to illustrate the essential characters, not only of the pathological states, but also of the normal balance between controlled and controlling forces which underlies the harmony of the healthy mental life.

Now that the war is over it becomes our task to utilise the knowledge we have gained from the study of a relatively simple and transparent form of disorder. We have to discover with equal clearness the nature of the psycho-neuroses which in times of peace take so large a place in the life and work of a physician. Just as the war neuroses are essentially the result of conflict between certain instinctive tendencies and the traditional sentiments and ideals of society concerning these tendencies, so can we be confident that a similar conflict will be found to form the essential factor in the psycho-neuroses of civil life. It will be our task to discover the nature of the instinctive tendencies and of the social ideals which form the opposing forces in this intestine warfare within the individual life. You all know how the most prominent school of students of the psycho-neuroses believe that the instinctive tendencies which stand on one side of the battleground belong exclusively to the instinct of sex. However repellent this may be to the traditions of the medical profession, we must be prepared to face this problem honestly and without prejudice. In turning from the practice of war to that of peace we must expect to find a great increase in the part taken by the sexual instinct, for the simple and obvious reason that the conditions of our civilisation make this instinct the special object of its repressions and taboos. We have found reason to believe that sex plays but a little part in the causation of war-neuroses, but it does not follow that this will hold good of the neuroses of civil life. On the other hand, we must be careful to hold the balance and not allow ourselves to give to sexual tendencies a prominence greater than they deserve. The sexual instinct is far from standing alone as the subject of the repressions and taboos of social tradition. It should be our working hypothesis that any instinct which needs repression in the interests of society may furnish that occasion for conflict which forms the essence of neurosis.

The psycho-neuroses form so absorbing a topic, and provide so vast a field for observation and speculation, that we may easily overlook another line of inquiry which should be of equal interest to our Section. There is an especial danger that we may overlook this second line of interest in that most of us have been engaged during the war exclusively in the study of those disorders of nervous and mental activity which we label 'functional.' Many perhaps may be hardly aware of the advance in psychological knowledge which has come from observations on the organic lesions furnished by the accidents of war.

The destruction of parts of the nervous system by injury or disease takes in the psycho-physiology of man the place which in the case of the lower animals belongs to experiment. We have to look to the investigation of organic lesions for light upon all those higher functions of the nervous system which give to man his distinctive place in the universe. Clinical investigation forms the chief instrument, sometimes the only instrument, which we can use in the investigation of speech or other of those higher reactions which are associated with mental activity.

The clinical investigation of the nervous system has in recent years thrown a flood of light upon all the sensori-motor processes which form the basis of mind. Pre-eminent in this respect have been the researches of Head. Working steadily through the nervous system from periphery to centre, he has now reached the cortex cerebri. In his recent paper on "Sensation and the Cerebral Cortex[1]" he has given us a clear and consistent account of the mechanisms by which the cortex exerts that selective and discriminative activity which we call 'attention.' It is only through such work that psychology can expect to gain any profit from the study of the anatomy and chemistry of the brain, whether normal or pathological. The neuron and the arrangements of neurons can have none but a purely speculative interest for the psychologist until the analysis of mental function has carried our knowledge far beyond the limits of the territory it has now conquered. It is only when such analysis as that carried out by Head in the case of cutaneous sensibility has been extended to other forms of sensation and to such manifestations of cortical activity as speech that we can expect to understand the nature of the relations between nervous and mental activity.

The experience of war has recently taken Head, working in conjunction with Riddoch[2], back to the spinal cord. It might be thought that there would be little here to interest us as psychologists, but the investigations of these workers have brought out many features of the functions of the spinal cord, when separated from the rest of the nervous system, which make a great addition to our knowledge. They contribute greatly to the solution of the problem, so vitally important to the psychologist, of the evolution of the reactions by which animals have adjusted themselves to the progressive increase in the complexity of their environment.

Especially instructive from this point of view is the discovery of the 'mass-reflex.' This is a mode of reaction of the isolated spinal cord, one of the most primitive kind, such as would ensure the bodily withdrawal of an animal from noxious stimulation. This reflex is so out of keeping with later modes of reaction that it has been wholly suppressed and buried, and its presence has only been revealed by one of the cruellest accidents of war.

Such clinical investigations as those of Head and his colleagues bring us to the physiology of the nervous system. Here again we have a subject which should take a prominent place in the deliberations of this Section. Especially through the work of Sherrington[3] we have been provided with a large body of knowledge which, though gained by purely physiological methods and purely physiological in character, should nevertheless be of great interest to the psychologist, for it reveals a general plan of neural function with which any plan of mental function must be in harmony. In work on the activities arising out of the complex relations of binocular vision Sherrington[1] has shown that his integrative mechanisms apply to a psychical process of a relatively high order. This should point the way to other work designed to bring out the relations between physiological and psychological integration.

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[1] *Brain*, 1918, XLI. 57.

[2] *Ibid.* 1918, XL. 188, 264.

[3] *The Integrative Action of the Nervous System*, London. 1906.

Another product of the recent physiology of the nervous system which I should like to mention is the 'all-or-none' principle, the demonstration of which we owe to Adrian and to Keith Lucas[2]. This principle, originally shown by these workers to characterize the activity of the isolated nerve fibre, holds good in large measure of the protopathic form of cutaneous sensibility and of the recently discovered 'mass-reflex.' Moreover, there is reason to believe that the 'all-or-none' principle applies to a large extent to emotional activity and to all those instinctive reactions which are emotional in nature. All these reactions have the character that, if they take place at all, they tend to appear in their full strength without that delicate discrimination of appreciation and graduation of response which are shown by reactions associated with intelligence.

The lines of inquiry from which these instances have been taken furnish so vast, and in some respects so highly specialised, a field that it might be thought they should form the work of a special Section of this Society, a Section of Physiological Psychology. I hope, however, that this step will not be taken, for I believe that it would be carrying the principle of specialisation to a harmful length. Rather would I advocate that this line of inquiry should be divided between this Section and that of Experimental Psychology, whenever such a Section is founded. I should like to see the physiology of the nervous system serving as a bond between the work of our Section and other branches of psychology. It has been a special feature of the experimental psychology of this country that, at any rate in its early days, it was clearly linked with physiology. I hope that this close relation will continue to form a bond linking our work with that of other Sections of the Society, so that they shall form an harmonious organization working with a common purpose, and with common principles, towards the better understanding of that which makes man what he is, which makes human society what it is—the Mind.

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[1] This *Journal*, 1904–05, I. 26

[2] *Brain*, 1918, XLI. 26.

SOME MEASUREMENTS OF THE ACCURACY OF THE TIME-INTERVALS IN PLAYING A KEYED INSTRUMENT.

By W. B. MORTON.

(*From the Physical Laboratory, Queen's University, Belfast.*)

§ 1. *Experimental arrangement and method.*
§ 2. *Playing 'three against two.'*
§ 3. *Spacing as influenced by accent and grouping.*
§ 4. *Accuracy and regularity attained in single intervals and groups.*
§ 5. *Accuracy and regularity as affected by rate of playing.*

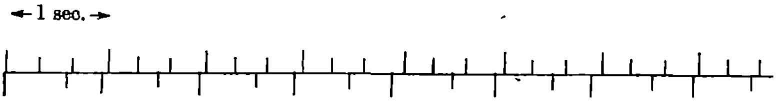
§ 1. THESE measurements were begun, some years ago, with the primary object of finding out the degree of accuracy attained by pianists in the feat of playing in double time with one hand and triple time with the other. The same experimental arrangement was later used to examine some other points which suggested themselves, in particular the influence of the accent on the spacing. The experiments were not carried out on any carefully arranged plan and were discontinued rather than finished. But some of the results obtained are perhaps worth putting on record.

The keyboard utilised for the purpose of the experiments was one of eight keys attached to a small wind-chest used for lecture experiments with organ-pipes. A brass pin was fixed into the end of each key-lever, and this pin rested in contact with a brass pillar, the contact being broken when the key was depressed. The contacts of the first four keys were included in the electric circuit of a chronograph tracer of the type used in physiological experiments, and the other four keys acted on a second tracer. The tracing points bore, side by side, on smoked paper carried on a drum revolved by hand, thus giving two close parallel lines. When a key was depressed the corresponding line was broken to one side. The two traces were crossed by the wavy line made by a point attached to a prong of an electrical tuning-fork making 128 vibrations per second. By counting, under a reading microscope, the number of tuning-fork waves between two breaks of the straight traces, the corresponding interval could be determined in periods of the fork and so in seconds. It was easy to estimate a tenth of a wave and so the time could be got to within about $\frac{1}{100}$ sec. The accuracy of the arrangement was tested by using a pendulum break and its errors were found to be small compared to the irregularities of the intervals measured. No attempt was made to reach extreme precision in the absolute values of the time intervals; so the fork was not tested for a possible deviation from its nominal rate. A metronome was used to give the player a preliminary idea of the rate at which he was to strike the keys, the metronome being stopped during the actual experiment. It was found that at M 120 for the bar, *i.e.* two bars per second, the number of waves counted to the bar did not differ sensibly from the theoretical value of 64.

It is of course obvious that the arrangement described could only reproduce very imperfectly the conditions of actual playing on a piano. The 'touch' was lighter and looser, and the sounds were wanting. It is doubtful how far this might modify the results obtained. I am inclined to think that the muscular movements concerned are too automatic to be much affected in their rhythm either by the sounds heard or by the reaction of the mechanism. It will be convenient in describing the experiments to ignore the dumbness of the keyboard and call each depression of a key a 'note.'

§ 2. In the initial experiments on 'three against two' I had the kind assistance of two friends who are professional musicians and good pianists. The two hands of the player acted on the two halves of the keyboard so that the movements were recorded on the two separate traces. The traces having been measured it was possible to obtain a visible representation of the playing by marking off the successive intervals along a line on squared paper. In this way the irregularities, even of the best traces, become glaringly evident to the eye. But even at the slowest rate adopted, of M 60 (one bar per second), the errors do not amount to more than about $\frac{1}{10}$ second and are, I suppose, quite unnoticed by the ear.

I reproduce a diagram of a portion of an average record which brings out an interesting point. This player's left hand was inclined to hurry on the right. In this case the right hand was playing in triple time (marked above the line) the left in double time (below the line). It will be seen that the first interval in the double time is longer than the second so that, in spite of the hurrying, the unaccented note of the double time bar shall come into place between the two notes of the triple time. This seems to indicate that the two hands are not really acting independently but that it is the pattern made by the combined systems of movements which is presented to the player's attention.



Right hand in 3-time. Left hand in 2-time. M 60.

§ 3. Coming to the general question of the regularity of spacing, one knows that the performance of a piece of music depends greatly for its expressiveness, and in particular for its 'phrasing,' on small variations of the *tempo* from the strict time-values indicated by the notes. This would seem to apply specially to a melody played on an organ where the performer is unable to give emphasis to particular notes by increased loudness. Considerations of this kind suggested the possibility of the spacing being affected by the fall of the accent, though it did not seem clear *a priori* in which direction the influence would be exerted. So experiments were made to test the point. At first I tried evenly spaced taps with one finger on a single key, the player grouping these mentally, first in twos and then in threes. The results were negative; no doubleness or tripleness could be distinguished in the sequence of measured intervals. However this failure may have been due to the conditions imposed by the defects of the apparatus. For it was necessary that time should be given for the key to make contact again between one tap and the next; this meant that the rate of playing had to be rather slow (about two notes per second) and the fingering very staccato.

To get over this difficulty the notes were next played by two fingers on the 4th and 5th keys so that the fingers acted on the independent circuits of the two tracers. Records were made by 21 different persons, who varied from practised pianists to one or two who required to concentrate their attention in order to bring the fingers down alternately. In general the second and third fingers were used, and the rate was M 120 for the bar, *i.e.* the interval between successive notes was about 32 vibrations of the fork. Each player made two records, accenting first the second finger and then the third. Examination of the traces did not disclose any universal rule. In some cases, generally those of the least expert players, the longer interval followed a definite finger, irrespective of the accent. But in the great majority of the remaining cases the shorter average interval followed the accent. In an extreme instance the average interval following the accent measured 26·5 vibrations and that preceding the accent 47·0, but generally the difference was not more than three or four vibrations.

After obtaining this result it occurred to me that the effect, if a real one, might be a matter rather of the grouping than of the accent. It is usual to regard the strong beat as beginning the group or bar so that the result might mean that a longer interval came between one group and the next than between the two notes belonging to one group. This seems a plausible supposition. Accordingly I tried a few experiments in which the player was told to group the pairs of notes in an iambic manner, weak-strong. The result was to confirm the above-mentioned surmise, for the shorter interval now preceded the accent. But it would require a more extended series of experiments to make certain of the precise relation of time-interval to grouping and accent.

§ 4. As regards the degree of accuracy and of regularity attained by the players an idea of this can be given by stating the average interval between the notes, the least and greatest values of this, and the 'standard deviation' (the square root of the average of the squares of errors from the mean). The accuracy of course varies very greatly with the expertness of the player. In the best case which I met with, the average interval (over 22 notes) was 28·7 vibrations, the least 26·7, the greatest 30·0, the standard deviation 0·96. At the other extreme I find an average of 32·7 varying from 22·9 to 46·6 with standard deviation 5·56 vibrations.

It is interesting to compare the regularity of the bars or groups of two notes with that of the single notes. Since the rhythm of the bars is what is given to the performer by the regular beating of the metronome, before the experiment, one would expect this to be maintained more steadily than the rhythm of twice the rate, into which it is divided by the separate notes. This proves to be the case in every instance I have examined. There is a tendency to follow an interval which is shorter than the average by one which is longer and so to preserve the length of the bar more nearly correct. In terms of the standard deviations the greater regularity of the bars is shown by the fact that the standard deviation of the bars is less than $\sqrt{2}$ (= 1·414) times that of the single intervals. To show this, let a = the average length of a single interval, $2a$ of the bar. Let the actual lengths of the n intervals be $a + x_1$, $a + x_2$ ... $a + x_n$, where n is the (even) number of intervals. Some of the x's are negative so that their sum is zero. The errors of the bars are $(x_1 + x_2)$, $(x_3 + x_4)$ ... $(x_{n-1} + x_n)$. Let s_1, s_2 be the standard deviations of single intervals and bars respectively. Then by definition,

$$s_1^2 = \frac{1}{n}(x_1^2 + x_2^2 + \dots + x_n^2),$$

$$\begin{aligned} s_2^2 &= \frac{1}{\frac{1}{2}n}\{(x_1+x_2)^2 + (x_3+x_4)^2 + \dots + (x_{n-1}+x_n)^2\} \\ &= \frac{2}{n}\{(x_1^2+x_2^2+\dots+x_n^2) + 2(x_1x_2 + x_3x_4 + \dots + x_{n-1}x_n)\} \\ &= 2s_1^2 + \frac{4}{n}(x_1x_2 + x_3x_4 + \dots + x_{n-1}x_n). \end{aligned}$$

If the positive and negative errors are distributed at random then when n is large the second term on the right is small and s_2 becomes approximately $= \sqrt{2} \,.\, s_1$. If s_2 is found to be less than $\sqrt{2} \,.\, s_1$ it shows a preponderance of negative terms in the bracket, and this implies that the consecutive x's multiplied together are opposite in sign; in other words, an interval which is too long is followed by one which is too short, so that the bar-length tends to be preserved.

Taking as examples the extreme cases quoted above, in the good case $s_1 = \cdot 96$, $\sqrt{2}s_1 = 1\cdot 36$, $s_2 = 1\cdot 12$; in the bad case $s_1 = 5\cdot 56$, $\sqrt{2}s_1 = 7\cdot 8$, $s_2 = 7\cdot 4$.

§ 5. In addition to the experiments carried out at a prescribed metronome rate, I obtained some records made by pianists who played a 'shake' with two fingers at their maximum speed. The rates recorded were in the neighbourhood of 10 notes per second, the average intervals ranging from 10·8 to 13·2 vibrations (one vibration = $\frac{1}{128}$ sec. approximately). There is a very great falling off in the regularity of the fingering at these high speeds. As compared with the values obtained at the slower rates of about 4 per second the standard deviations are much greater, not merely relatively to the interval, but in absolute value. For example, the player whose deviation was only ·96 of a vibration with a time-interval of about 30 showed a deviation of 2·12 when the interval was 12·4, the extreme values recorded being 8·2 and 17·8. Traces made by another pianist gave, at the slow rate (M 120), an average interval of 31·1, extreme values 27·5 and 33·3, standard deviation 1·35; at the fast rate with average interval 10·8 the extreme values were 7·6 and 13·4 and the standard deviation 3·91.

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SOME EXPERIMENTS IN LEARNING AND RETENTION.

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I. Bergson's distinction between habit and memory.
II. Description of the tests.
III. Conditions of Experiment.
IV. Method of working out the results
V. Results.
VI. The part played by conation in learning.
VII. Is there any improvement in retentiveness?
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I. BERGSON'S DISTINCTION BETWEEN HABIT AND MEMORY.

THE following experiments represent an attempt to obtain by the method of experiment direct evidence of the influence of conation in some of the learning processes and to show in those processes its relation to mechanically determined associations. The investigation started from the contention of Professor Bergson that the word 'memory' is too frequently used as equivalent to the power of forming mechanical associations, whereas such a use overlooks those recollections which in their very nature are unique experiences, but which are not the less effectively reproduced. "The past survives under two distinct forms: first in motor mechanisms, secondly in independent recollections.... The memory of a lesson remembered in the sense of learnt by heart has all the marks of a habit...; the memory of each successive reading has none of the marks of a habit....It may be urged that these two recollections, that of the reading and that of the lesson, differ only as the less from the more and that...the lesson learnt is but a composite image in which all the readings are blended....But it is no less certain that each of them considered as a new reading and not as a lesson better known is entirely sufficient to itself....Of these two memories one is memory pure, the other is habit interpreted by memory[1]."

In most of the experiments undertaken to study problems of memory there has been an almost exclusive attention to those forms of retention which are described by Professor Bergson as being of the nature of habit, *i.e.* they have depended on the formation of associations by frequent repetition and have then been interpreted as holding true of all retention[1]. In actual experience it is not possible to fulfil all Professor Bergson's theoretical conditions, as the two factors called by him 'habit' and 'pure memory' co-operate in any given concrete case; but it is possible experimentally to bring about experiences which approximate to one rather than to the other. If all memory is of the habit type, then all tests which involve to any extent retention ought to correlate with those in which motor habit plays an obvious part, or at least they should correlate as highly with such as with others. If on the other hand Professor Bergson's distinction is fundamental, then we should expect the tests concerned with retention to fall into two groups; those of the habit type should correlate together and those predominantly involving pure memory should correlate together; but success in the one group would not necessarily carry along with it success in the other, *e.g.* we might find that people markedly good at tasks of the one type are weak at those of the other.

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[1] Bergson, *Matière et Mémoire*, Ch. II.

The tests chosen were intended to illustrate both modes of retention, *i.e.* reproduction of unique experiences and reproduction after repetition.

II. DESCRIPTION OF THE TESTS.

The Blot Test. Ten cards on each of which was a large ink blot of irregular shape were shown to the subject in succession for 5 seconds each. These cards were then shuffled with 10 more of a like character which the subject had not seen. The 20 cards were then shown to the subject successively, at the rate of one card per two seconds, who was asked to say 'yes' or 'no' according as he had or had not seen the card before. Errors were of two kinds, *viz.* cards falsely recognised as having been seen, and cards unrecognised which had been seen. In working out the results the two kinds of error were not kept separate.

The Picture Test. In this a simple picture was shown to the subject for 10 seconds, at the end of which time he was asked to write a sufficiently detailed account of the picture to enable any person, with the requisite executive ability, after reading the written account to draw the picture. The pictures chosen had not previously been seen by the subjects. In selecting the pictures it was essential that they should be clear and unambiguous, without much detail and with definite colours; and it was found advisable to avoid anything which might offer an arithmetical challenge, *e.g.* rows of nails on a box, as some subjects tended to spend all their time counting.

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[1] See McDougall, *Body and Mind*, Ch. xxiv.

Most of the errors were errors of omission. For marking purposes the picture was divided into a number of units, and one mark was awarded for each correct unit.

The Prose Test. A passage of prose was read through slowly once, after which the subject was asked to reproduce the substance in detail, aiming at the meaning rather than at verbal reproduction. For marking, the passage was treated as the picture[1].

Learning of Nonsense Syllables. Lists consisting of ten nonsense syllables were used. The lists were clearly printed on a card and each syllable was presented through a hole in a sheet of cardboard of such a size as to exhibit one syllable only. The experimenter moved the card so as to present each syllable to the subject for one second, and the subject was instructed to read the syllables aloud as they appeared and to make an effort to learn them by heart. The number of repetitions required to learn the series was noted. The syllables were all constructed mechanically and pains taken to avoid those suggesting any meaning.

The Typewriter Test. The typewriter used was an old-fashioned "Hammond" with black keys resembling those of a piano. Three keys (*a*, *b* and *c*) were distinguished from the others by being painted a light colour. The subject was instructed to start at the middle key (*b*), then to strike the marked key to the right (*c*), then to strike the middle key (*b*), then the one to the left (*a*) and back to the middle key (*b*), keeping the elbow rigidly extended[2]. The two keys (*a*) and (*c*) were not equidistant from the middle key (*b*) which was the 'space key.' The result of these movements was to print two letters on the paper. After the letters had been printed five times, the subject was told to close her eyes and to continue the movements which, if correct, she was allowed to continue until the letters had been correctly printed five times. If, however, she made a mistake, she was told to open her eyes and to print the letters again five times and so on. The number of repetitions required before the movements could be repeated automatically, was taken as a measure of the facility of habit formation[3].

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[1] Cf. Whipple, *Manual of Mental and Physical Tests*, 398.

[2] It was found necessary to insist on this as otherwise some subjects changed their method during the test; by insisting on this rigidity greater uniformity was obtained and higher reliability coefficients.

[3] This test was done first with the right hand and then with the left, but as there was great uniformity of place position in the results, the two have been taken together in working out the correlation.

III. CONDITIONS OF EXPERIMENT.

The subjects of these experiments were 41 women students and the experiments all took place at the same time every day, *viz.* between 1.30 and 3 p.m., each subject coming three days in succession and doing a complete set of the five tests each day. In all forty-one adults completed the sets.

IV. METHOD OF WORKING OUT THE RESULTS.

The method of correlation used was the Product Moment Formula applied to ranks[1]. To get the reliability coefficient (Rel. Coef.) Dr Spearman's suggestion[2] was adopted, the results of the first and third day being amalgamated and their correlation with the second day's results worked out.

The probable error (in brackets) of the uncorrected coefficient was worked on the formula

$$\frac{\cdot 7063\,(1 - r^2)}{\sqrt{n}}$$

V. RESULTS.

	Blots	N.S.	Pict.	Pr.	T.	Rel. Coef.
Blots	—	·05 (·11)	·53 (·07)	·26 (·10)	– ·05 (·11)	·50
Nonsense syllables ...	·05	—	·03 (·11)	·48 (·08)	·61 (·07)	·68
Picture	·53	·03 (·11)	—	·22 (·10)	– ·11 (·11)	·69
Prose	·26	·48 (·08)	·22 (·10)	—	·34 (·09)	·65
Typewriter	– ·05	·61 (·07)	– ·11 (·11)	·34 (·09)	—	·65

The other results with a group of 12 children are:
Recognition test and 8 nonsense syllables ·03 p.e. ·19.
" " " 10 " " – ·03 " ·19.

Of these tests the 'blots' and the 'picture' approximate to what Prof. Bergson calls 'pure memory,' inasmuch as each represents a specific experience unique in the history of the subject; there could be no question here of learning by repetition.

On the other hand, in the learning of the nonsense syllables and the fixing of the movements of the typewriter test there are all the characteristics of habit formation[3].

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[1] W. Brown, *Mental Measurement*, 48–49.

[2] This *Journal*, III. Pt. 3.

[3] Bergson, *Matière et Mémoire*, Ch. II.

When we turn to the table of correlation we find that these two, the nonsense syllables and the typewriter, correlate together highly, *viz.* ·61, while the two tests where the habit factor is at a minimum, *viz.* the 'blots' and 'picture,' correlate together to the extent of ·53; but between each member of this latter group and either member of the other groups there is an absence of correlation, *viz.* ·05 and − ·05 respectively for the 'blots' with the nonsense syllables and the typewriter and ·03 and − ·11 for the 'picture' with the nonsense syllables and the typewriter. We thus seem to get two well-marked groups; (*a*) those tests of what Prof. Bergson calls 'pure memory' dependent on the meaning of the whole, which, treated statistically, show high correlation; (*b*) those tests showing all the characteristics of habit formation, which correlate with each other but not with either member of the other group.

Turning now to the 'reproduction of prose' test, we see that it shows an affinity with both groups, *viz.* ·26 with the 'blots' and ·22 with the 'picture,' and on the other hand ·48 and ·34 with the nonsense syllables and the typewriter test. In analysing the results there seems to be a considerable amount of literal reproduction: the passages chosen contained many well-known phrases, *e.g.* "a few years ago," which by the majority of subjects were reproduced *verbatim*, *i.e.* they represent verbal habits; and such reproductions amount to 39 per cent. of the total. The substance, however, as a whole was new to the subjects and so approximates to the non-mechanically determined type. This test is interesting as showing the interrelation of the two types in one experience.

One point of practical importance might well be considered here. To a degree greater than is generally recognised, learning at school resolves itself into the formation of mechanical associations between symbols, *e.g.* the association between the written and spoken symbol in reading, or between the corresponding symbol in another language, the learning by heart of tables, declensions, etc. All these must, for effective use, become mechanical by constant repetition. Now some children seem to find the making of such mechanical associations extremely difficult and hence will be handicapped in most of the subjects of a school curriculum (the exception will probably be scientific studies and handwork); such children may gain a quite undeserved reputation for stupidity[1].

Again it is possible that some of the discrepant results obtained in the investigation of formal training may be due to ignoring this distinction: if the tests used depended mainly upon the power to form mechanical associations, a transference might be shown with this as a common factor.

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[1] This will be particularly so when the curriculum gives a disproportionate amount of time to linguistic subjects; in such a case a child may learn to believe in its apparent stupidity and lose self-confidence.

VI. THE PART PLAYED BY CONATION IN LEARNING.

Mechanical association having been shown to be but one factor in retention, it remains to be investigated whether the success of the learning process is affected by reliance on this factor. That is to say, is learning more successful when conation or the will to learn is operative, or when the influence of quasi-mechanical repetition is allowed full sway? The material forming the subject matter of these experiments was nonsense syllables, chosen because a sequence of such syllables has, as a whole, no meaning (hence it is possible to avoid the complication of the uncertain and incalculable operation of varying interest), and also because an indefinite number of sequences of equal difficulty can be produced. For learning purposes the syllables were printed on strips of paper fastened round a cylindrical drum which could be wound up to revolve at a given speed. A piece of cardboard with a window cut in it, placed in front of the drum, permitted but one syllable at a time to be seen; the machine was arranged to show each syllable for one second. The learner noted each reading of the set and kept a record of the number of repetitions required before the sequence could be reproduced by heart.

The standard row for these experiments consisted of twelve syllables and the writers of this paper were the subjects except when a statement is made to the contrary. The experiments fall into two groups according to the attitude of the learner; in one group the learner makes an effort to learn the sequence of syllables as quickly as possible, while in the other he relies on learning them by mechanical repetition, maintaining an attitude of passivity towards them except in so far as each one is read aloud as it appears. This passive attitude required much practice on the part of both subjects before it could be successfully maintained, and subjectively it was a less interesting mental state. Even the most rigidly constructed set of nonsense syllables acquires some meaning if the mind is allowed full activity; *e.g.* the place positions of certain syllables are noted, or some syllables are more interesting and pleasing than others. The learning is a problem the satisfactory completion of which is important, so that the total experience, while not perhaps to be described as thrilling, is nevertheless interesting. But in learning the series passively, all this excitement was absent, and the whole experience seemed 'flat.' After some experience both subjects could adopt the passive attitude at will.

Averaging about 20 experiments of each type we find in the case of one subject that 13 repetitions are required to learn rows of nonsense syllables when making the maximum effort to do so, and in the case of the other subject that 9 repetitions are required. When however no effort is made and reliance is placed only on the formation of mechanical associations by repetition, an average of 89 repetitions is required for one subject and 100 for the other. That is, from seven to eleven times as many repetitions are required to learn the same number of syllables by heart.

Again, if in the case of passive learning we compare each subject's performance at the beginning with his performance after some months' practice, we find a gradual increase in the number of repetitions required to learn each row; in the case of one subject the increase is from 39 to 165 repetitions and in the case of the other from 45 to 204, an increase dependent upon the increasing success with which the passive attitude was maintained. In other words, the greater the passivity and therefore the greater the rôle of mechanical determination, the greater the number of repetitions required, *i.e.* the less successful the learning.

But it may be that, when the learning has once been effected by this great expenditure of time, the material learnt is retained more effectively than when learnt actively; and there may be some such advantage in mechanical or passive learning. Now the test of effective retention is the success with which the material learnt can be recalled, and for experimental purposes the measure of that success is the number of repetitions required to relearn the same material after the lapse of a given interval. A comparison can thus be made between the number of repetitions required to relearn actively a set of nonsense syllables which has been learnt actively and the number of repetitions required to relearn actively a group which originally was learnt passively. If as many repetitions are required to relearn a group after an interval as are required to learn it at first, then the original learning has for practical purposes been of no avail: if the original learning has been perfectly retained then no repetitions would be required to relearn, *i.e.* the sequence learnt could be recalled at will.

Turning now to these experiments we find that, averaging 20 attempts in the case of one subject, nine repetitions are required to learn a set of syllables when an effort has been made and that four repetitions are required to relearn after an interval of 24 hours; *i.e.* there is a gain of five repetitions representing the effectiveness of the retention. On the other hand, we find that when no effort has been made, an average of 99 repetitions for learning requires an average of five repetitions to relearn; *i.e.* the retention is not as effective as when the original learning has been done actively, although there were considerably more repetitions. Averaging 20 attempts in the case of the other subject, we find that 13 repetitions are required to learn the syllables actively and 90 to learn them passively, and that to relearn the same series after an interval of 24 hours takes eight and seven repetitions respectively; *i.e.* the passive learning does in this case require one less repetition for relearning than the active learning, but the difference is not proportional to the extra time taken.

Subjectively there was, with many of the series learned passively, a feeling of absolute unfamiliarity when relearning began; whereas on relearning after having learnt a list actively there was always a recognition that the series had been experienced before.

This however is after a short interval; it may possibly be that, when the time interval is longer, the frequent repetition involved in the passive learning may have the effect of preventing the decay of the material learnt. To test this some experiments were made to measure the influence of the time interval. The contrasting time intervals were, (*a*) between 7 and 18 days for the long interval, (*b*) under 24 hours for the short interval.

(*a*) The records of two subjects were obtained. In the case of one subject 10 repetitions were needed to learn several series actively, and 6 repetitions for relearning after a long interval: 40 repetitions were required for passive learning and 9 repetitions for relearning[1]. The other subject required an average of 8 repetitions for learning actively and 5·6 repetitions to relearn the same after a long interval; when learning passively he required an average of 172 repetitions for learning and 7·5 repetitions for relearning.

With both subjects after a long interval the loss is greater when the initial learning has been passive than when there has been the will to learn; so that there is little difference between the number of repetitions required for relearning what has once been learned passively and the number of repetitions required to learn for the first time; whereas when the original learning has been active there is clear proof of retention. The effects then of passive learning wear off much more quickly.

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[1] The figures state the averaged results.

(*b*) When the time interval is short, *i.e.* less than 24 hours, we get the following results. One subject requires an average of 10 repetitions to learn a series actively, and an average of 6 repetitions to relearn, and an average of 41 repetitions to learn a similar series passively and 4 repetitions for relearning; here there seems to be some superiority in the passive learning. The other subject requires an average of 7·5 repetitions to learn several series actively and 4 repetitions to relearn; and an average of 90 repetitions to learn similar series passively, and 5 repetitions to relearn; in this case there is an advantage on the side of the active learning.

Judged then by the standard of effective retention and recall over both longer and shorter intervals, passive learning stands condemned. Consequently the effort to learn plays a very real part in the learning process, and to assume that learning consists either solely or chiefly in the formation of verbal habits by mechanical association is to assign to the least important factor the dominant place. In only one case was there any superiority in the retentiveness after passive learning, and that was when the interval was short.

VII. IS THERE ANY IMPROVEMENT IN RETENTIVENESS?

All investigators of the learning processes have realised the somewhat irregular but general improvement which takes place if the same type of active learning is continued over some considerable period. This improvement, which may be due to improved concentration on the task, or to better realisation of the problem or of the means to attack it, is generally described as improvement of memory by practice[1]. Now it is easy to measure this improvement, as it is only necessary to compare the average of the repetitions required to learn several series of syllables at the beginning of an investigation, with the number required to learn the same number of series after some months of practice. But can we measure also whether with practice there is any improvement, not only in the power to learn but also in the power to retain? Improvement in retentiveness may conceivably prove to be a factor even in the improvement in learning; but if there be such an improvement it will show itself more purely in a measurement of retention. Evidence of it may be sought by comparing the repetitions required for relearning at the beginning and at the end of a given period of practice. We shall have, of course, the improvement due to improved methods of learning as a factor common both to learning and relearning; and if any improvement of relearning is exactly proportional to the improvement in learning, we may legitimately conclude that the improvement in both cases is the expression of the improved methods of learning, rather than to the improvement in retention. But, if there be a disproportionate improvement in relearning, so that relatively there is greater improvement in the relearning than in the learning, we may conclude that that improvement is due to a factor peculiar to the relearning process, which factor can only be improvement in the power to retain[1].

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[1] The most elaborate investigation of this kind is that of Prof. Meumann and his collaborators. It is claimed that these experiments show great improvement of memory through practice. But the method does not attempt to distinguish between improvement in application to the task and improvement in retentiveness and we do not know of any attempt to measure improvement of retentiveness previous to our own.

For these particular experiments six subjects' records are available. The period of practice for five subjects was six months, during which almost daily practice was given; and in the case of one subject the period was twelve months. The general method of procedure was as follows. On one day a group of syllables was learnt, and 24 hours afterwards that same group was relearnt, and so on. Below is a table of the results:

Subjects	Av. of 8 expts. giving no. of repetitions for learning at beginning	Av of 8 expts. giving no of repetitions for learning 6 months after	Gain	Av. no. of repetitions to relearn at beginning	Av. no. of repetitions to relearn 6 months after	Gain
A.	14	8	43 %	7	4	43 %
B.	16	9·6	40	5·6	3	47
C.	8	7	—	3	3	—
D.	9	5·6	38	2·8	1·2	57
E.	13	11	15	9	6	33⅓
F.*	12	8	33⅓	6	4	33⅓

* Interval 12 months.

It will be seen that the subject C. improves very little in either task; this subject was particularly good at all learning by heart and habitually relied upon the power.

Subjects A. and F. appear to make a proportional improvement both in learning and relearning, but in B., D. and E. there is more than a proportional improvement in the relearning; the improvement in learning being represented by gains of 40 per cent., 38 per cent. and 15 per cent. respectively; whereas the improvement in the relearning is represented by gains of 47 per cent., 57 per cent. and 33⅓ per cent. It seems then as if in some persons at least, practice in memorising produces improvement not only in learning but also in the power to retain what has been learnt[1].

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[1] It should be noted that no effort was made during the intervals to recall consciously the learnt syllables; any temptation there might be to do this rarely survives the first few experiences of learning nonsense syllables, and where the experiments extend over some years such extra repetitions can be ruled out.

VIII. CONCLUSIONS.

In this paper we have adduced experimental evidence in support of Prof. Bergson's distinction between habit and memory; we have illustrated the great importance of effort or volition in rendering repetition effective in memorising; we have made experiments which constitute, we believe, the first attempt to investigate the question—Does practice in memorising produce improvement of retention as well as of the power to commit to memory?—and we have obtained results which seem to show that in some persons such improvement may be produced.

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[1] For a discussion of the more philosophical considerations see McDougall, *Body and Mind*, Chs. XXIII and XXIV.

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THE PRESENT ATTITUDE OF EMPLOYEES TO INDUSTRIAL PSYCHOLOGY[1].

BY SUSIE S. BRIERLEY.

- *I. The opposition among educated workers.*
- *II. The need to consider larger social relations in order to understand motives in industry.*
  - *(a) The importance of maintaining the autonomy of psychology.*
  - *(b) The fallacy of the 'economic man.'*
  - *(c) The danger of over-emphasis on 'production' as the purpose of psychology in industry.*
- *III. The main factors in opposition.*
  - *(a) Suspicion.*
  - *(b) Jealousy for the solidarity of workers.*
  - *(c) Fear of increased monotony.*
  - *(d) Dread of loss of craftsmanship.*
  - *(e) Emphasis on the value of human personality.*
- *IV. Justification, in actual social conditions and in recent psychology, of the demand for self-fulfilment in work.*
- *V. Divergent tendencies in the 'Great Industry' and the 'Great Society.'*

I.

THE attention of psychologists has recently been drawn to the widespread opposition to the introduction of psychological methods into industry, existing among organized labour. It is found to be not the least strong among the more educated groups, and the importance of gaining the goodwill of these groups cannot be over-estimated, since they exert great influence upon Trade Union policy and general Labour opinion. The present paper is an attempt to state the workers' point of view, and to develop its psychological bearings, without making final judgment as to the rightness or wrongness of the detailed criticisms they offer. Their antagonism may be due in part to sheer misunderstanding; but it will be found also to raise many fundamental difficulties, which it is imperative that we should consider.

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[1] A paper read before the Industrial Section of the British Psychological Society on 28 November, 1919.

II.

It will be seen that it is impossible even to state the issues without taking into consideration certain controversies in ethics and economics. Some of the influences determining the attitude of the workers without doubt have their root in current social conflict. That does not, however, absolve the psychologist from stating and weighing them as psychological factors. It cannot be said that there is one set of events and issues which belongs exclusively to economics, another to ethics and another to psychology. There are no elements in human experience which have not their psychological value, whatever other values they may carry at the same time. In all that follows, then, I wish, for the purpose of this paper, to make no pronouncement as to the issues involved, save in so far as they are psychological issues.

It has to be noted at the outset, that it is by no means easy to get at the real minds and motives of the workers within the workshop itself, since there inevitably come into play instinctive defences again anyone, no matter how friendly and tactful, who does not belong to the workers' own social group, and still more so if he belongs in any capacity to the employing group. The workers themselves have assured me that this is in part deliberate; but there is no doubt that it is mainly instinctive and sub-conscious. Mr Ordway Tead has remarked[1] how he visited "a meeting of a local Trade Union in the Middle West, in the evening after an investigation of the shop in which nearly all the men worked: and the most noteworthy impression derived from the gathering was of the initiative and ability of men who in a conference held in the forenoon with the shop foreman had hardly dared to open their mouths"; and this is the experience of many students of working men. Whatever the psychological mechanisms involved, there is no doubt that the employer or investigator within the workshop is often unable to penetrate to any real extent the determining tendencies of the workers' minds.

(*a*) It may be replied to this that the psychological expert, as distinct from the employer, is exempt from the effective operation of these defences, by virtue of his appreciation of their mechanism; that while it may be true that the worker tends to regard him as belonging to the camp of the enemy, yet this hostility does not shut out the psychologist from an understanding of the worker's mind, since he is able to appraise that very hostility as indicative of conscious and unconscious motive. This indeed ought to be true. It is true in so far as the psychologist himself is free from certain assumptions inherent in the relations between capital and labour in industry; in so far as he insists on the obvious truth that the world of industry is not self-contained and self-sufficient, and cannot have a psychology unto itself; in so far as he claims not merely the right of impartiality between employer and employed concerning particular industrial problems, but the further right to consider these problems in relation to the psychology of the larger human world of society as a whole. If he is "above the battle" in this most comprehensive sense, the industrial psychologist will be able to penetrate the fighting defences of the workers and gain a reliable understanding of their motives and purposes.

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[1] Ordway Tead, ***Instincts in Industry***, London, 1919.

If now we ask how far this essential independence and integrity are maintained, I am not sure that we are being entirely successful. You will understand that I am not accusing my fellow-psychologists of partiality as regards any conflicting interests of capital and labour in particular issues. Such a charge would be absurd and untrue, and I need scarcely trouble to disclaim it. The danger that I am inclined to suspect is from the point of view of our science perhaps a graver one. It is that we tend to allow the immediate practical needs of industry to dominate our outlook so much as to diminish our scientific autonomy. By so doing, it is true we achieve a simplification of our problems, convenient for temporary purposes. It is however a deceptive over-simplification, detrimental to the truth of our science and ultimately to its social usefulness.

(*b*) To consider an important example.—There is much to show that we are subtly influenced in our reading of industrial problems by a conception which is still generally current in the industrial world, and in many circles outside industry, the classic conception of the 'economic man.' That conception of a mannikin whose behaviour is actuated solely by one spring, the 'economic motive,' may or may not be a useful basis for the science of economics. It has been the traditional view that just as the mathematician, the physicist or the chemist must of necessity work with a single aspect of nature, considered in abstraction from all other qualities in the manifold of experience, so the economist must deal with this single abstracted aspect of human nature. Having worked out its implications and fully developed his science on this simple basis, he may then return to the complexities of actual experience, and modify his formulations in accordance with the full qualities of psychological fact. He may then make the necessary adaptations imposed by motives other than economic, and determined by the total of ethical and social purposes. Whether this high-minded procedure of the economist is sound and legitimate is however itself a psychological question.

I do not propose to develop here the question of its value for the science of economics. I am concerned with the fact that it is an actual working assumption, widely current in industrial thought, and one which appears to have exerted a real and perhaps unsuspected influence upon the psychologists' point of view. Its acceptance by psychologists is of course nowhere explicit, for merely to state it is to see its psychological falsity. The economist may possibly be able to argue *as if* that were true, but no psychologist can possibly subscribe to such a view, on deliberation. We may admit that self-preservation is one of the basic motives, possibly *the* basic motive, of human conduct. But the self that is jealously guarded, in a society so complex as our own, is much more than the mere bodily self that requires daily bread; there are ways of preserving it other than those that are referred to under the name of 'economic motive.'

Yet the industrial psychologist has not explicitly repudiated the economist's conception, and it is indeed possible to infer from his general pronouncements that he is subtly dominated by the supposed adequacy of the economic motive. For we do naïvely reply to inquirers as to the workers' attitude to our innovations, "Oh, they are quite content, for they have shorter hours and bigger wages!" appearing to imply that this is the essential key to the problem. I am not of course denying significance, and very great significance, to this factor, especially among the lower ranks of labour. I am deploring that we should give it single or undue prominence. I believe such a view to be a profound misunderstanding of the facts of the case, as regards those whose opposition will be the most telling, and the most difficult to remove.

(*c*) A further point in which I am not sure that we are maintaining the proper independence of our science, and one that lends colour to the plausibility of the economic motive, is in regard to our industrial aims. The partial purpose of 'increased production' has been too unquestioningly accepted as the reason for the psychologist's incursion into industrial method, and may too exclusively determine the scope of our studies. Again let me make a disclaimer. I am not challenging the necessity for increased production. No one can doubt its extreme urgency in the present social and international situation. Nor am I denying that the necessity is a legitimate and sufficiently good reason for the improvement of industrial methods. What I suggest is that our horizon as industrial psychologists must not be bound by this circumscribed end. We must put this into perspective, we must see it in its proper relation to the whole of which it is but a part. For from the larger social standpoint, the labourer is not merely a factor in production, but himself gives meaning to the process of production. Not only are his material needs to be met directly or indirectly by what is manufactured, but his life, the life of a human being, is to be expressed in the social medium that his work conditions provide. The office, the factory and the workshop are part of the field in which men and women live out their human relationships, part of the means by which citizens exercise their civic functions, part of the education by which they grow to full moral height. And the psychologist must remember these wider aspects if he is to understand the workers' responses. He must indeed face the whole problem of human nature in industry. He must not only serve partial ends, but must also ask, what is the significance for the worker's whole personality of his industrial actions and reactions. He must inquire how far labour conditions help, and how far they hinder the healthy and balanced expression of the deepest needs of a complete human being. The greater as well as the lesser purposes of human social life command the services of psychological science.

III.

We must, then, set on one side any presuppositions dictated to us by economic or other theories and practices, and approach our problem upon the broadest human basis, which is indeed the only basis for an adequate psychology. From this standpoint I wish briefly to discuss what appear to me to be the main factors determining the attitude of the workers to scientific improvements in management. I shall do so as briefly as possible, for I am anxious to have full discussion of the points raised.

The more straightforward issues I shall do little more than mention, as, *e.g.*, habit, and the general fear of the unknown. These are certainly operative, but their influence is not specific to our particular problem.

(*a*) A potent factor is suspicion, on the part of the workers, of the motives actuating the attempt to change industrial practices. The workers look for what they call 'the ulterior motive' behind every move on the part of the employer, or those working on behalf of the employer, to better the conditions of labour. It is often extremely hard to convince them that the ostensible motives of justice, efficiency and human decency are in truth the actual motives. The psychology of this is pretty clear. The suspicion is partly due to the normal functioning of the herd-impulse, when the group is in conflict with other groups, and partly to a reflective wariness,—the outcome of experience. The psychologist undertaking to reform industrial method thus enters upon an unfortunate legacy from historical conditions. This distrust is moreover subject to a pathological exaggeration, which itself serves to intensify the antagonism. Our concern cannot cease, however, with the recognition of a pathological element in the labourer's attitude. We must go on to ask what are its determining conditions; and in answering that question we shall inevitably be driven to survey the widest issues of social reactions in a complex industrial civilisation. Here we can only note in passing the suggestion made by Mr Graham Wallas, and developed by Mr Ordway Tead, in special reference to the life of the industrial classes, and supported also by many general considerations of pathology and physiology, namely, that it is the dispositions which that life 'baulks,' the instincts which it represses, that give rise to the pathological element in their reactions.

(*b*) A third factor determining resistance to psychological reforms in industry is the commonly recognised one of fear of unemployment. The interest of this motive for us, however, is not with regard to practical remedies, but concerns its relation to the larger and more intricate problem of the workers' solidarity. The term 'solidarity' often has a non-psychological meaning, that of the real or supposed unity of the social and economic interests of the wage-earning, as opposed to the property-owning classes. This is the significance which it frequently has for economic theorists and propagandists, although it may carry an indistinct psychological flavour at the same time. I am using it to refer simply to the psychological fact of actual awareness of unity, of behaviour loyal to those real or supposed interests. A study of this behaviour, of the manner, the emotional expressions, and the arguments of work-people in this regard reveals the familiar and characteristic working of the herd-spirit. In relation to the opposition to reform in industrial method, working-class solidarity manifests itself in two ways.

In the first place, there is with many men not only a fear of personal displacement as a result of changes in industrial conditions, but also a fine loyalty to those poorer in capacity and in market value than themselves. Secondly, and perhaps more important from our point of view, there is a dread of anything that holds possibilities of the weakening of Trade Unionism and of danger to the hard-fought-for principle of collective bargaining. These are the sources of resistance to the introduction of, *e.g.*, differential rates of payment, premium bonus systems, etc. We cannot doubt that there is often an element of true generosity in the impulse to protect the weaker fellow. That is to say, psychologically speaking, its altruism has its unconscious roots not in the welfare of the particular limited and temporary group we are considering, but in wider, deeper, more ancient and permanent social pressures. Yet the student of human nature would find it hard to believe that a simple generosity towards inefficient fellows could ever prove strong enough to compel the highly skilled workman to resist, say, a system of payment that would be to his own distinct advantage. Such generosity would be merely quixotic if it occurred. The generous impulse must derive the greater share of its strength and poignancy from the fact that the inefficient workman *is* a workman. As such he has a significant bond with his high-grade fellow,—a bond considered to be more vital than that with any other economic group. Here again there is a rational element. The high-class and successful worker knows well the fortuity of those circumstances which, often independently of native worth, serve to bring about final differences in market value. He knows the enormous effect of early training, and the inevitable deterioration in social and economic worth that is brought about by long periods of involuntary unemployment, by diseases of occupation, by countless influences outside the will of the individual. The potency of the impulse of generosity is thus conditioned by the strength of the group-spirit of labour, and this again interacts with the impulse of self-preservation, and in fewer cases with devotion to the wider welfare of the whole community. The actual behaviour of the workers as regards the aspect we are discussing will be determined by judgments made at the behest of all these motives, which vary in relative strength among individuals. The clear demonstration of high personal advantage is sufficient to win the co-operation of many. Others will need to have it shown that the whole labour world will benefit permanently from the innovations. There can be no doubt of the relative ethical value of these two attitudes. My own experience leads me to think that it is precisely among the most intelligent and best-informed workers that the opposition to any method likely to weaken the general organization of labour will be the strongest and the most persistent. The sudden distraction of immediate self-interest for its leaders may suffice to disperse a mob. But where the reflective element is more prominent as a basis of grouping, the herd is likely to be more tenacious. The educated workers know how intimately the progress and welfare of their class have depended on group-organization, and they understand moreover that the general good of Society is enhanced by improvement in the standard of life of its labourers. The mere herd-impulse is thus with them largely transformed into an intelligent conjunction of purposes. In order, therefore, to win the goodwill of the most valuable sections of the working world, industrial psychologists need to take the widest possible survey of their problem. They need to base their exposition and appeal upon a consideration of these far-reaching issues, as well as on immediate and apparent advantages. I am indeed inclined to think, apart from the matter of the worker's goodwill, that the serious attention of applied psychologists should be given to the question of the influence of any particular industrial method on Trade Unionism and the group-spirit of the workers. This is one of the points in which we are bound to face responsibility for the social and economic effects of our suggestions, over and above their technical psychological merits and immediate effect upon production.

We are not, of course, directly responsible for the adoption or rejection of our methods. That is the concern of the industry and of the public. In our capacity as experts, we are responsible for making quite clear to the public mind the full psychological effects, in all directions, of our proposed developments of method.

From the point of view of political economy, there seems to be no doubt that a strong Unionism and a tenacious hold on the principle of collective bargaining is essential to the welfare of Labour, and hence to that of the community. And from the ethical standpoint, loyalty and devotion to the interests of labour groups is a fruitful education in public service. There is indeed a great field of psychological inquiry waiting to be entered upon here with regard to the relative social and educational values of the different minor groups to which the individual may give allegiance within the 'Great Society' itself. The question of the social value of those particular groups we call Trade Unions is much affected by the problem of craftsmanship which we are presently to consider. In so far as the Unions are Craft Unions, their function approaches that of 'professional' organizations of teachers, lawyers, medical men and so forth, the social usefulness of which cannot be doubted. For they are not concerned with economic relations alone, but maintain the honour and skill of their craft and provide a true education in the service of Society. But the tendency of developments in industrial technique is such as to destroy craftsmanship, turning Craft Unions more and more into Class Unions, concerned only with economic bargaining. The dangers and evils of sectional grouping are thus put at a premium and its benefits minimised.

(*c*) We come now to the last group of factors determining the workers' opposition. These are factors the discussion of which raises far the most fundamental difficulties, and whose significance we cannot begin to appreciate without going out beyond the bounds of the workshop into social and civic relations. We may perhaps summarise this set of influences by saying that the workman, stimulated by the growth of political democracy and by a measure of education, has arrived at and holds firmly to a conception of his own human dignity and of the basic value of human personality. This is now so vital to him that he dreads and resists any industrial development which appears to him to hold possibilities of increasing the already crushing effects of industrial life upon that personality.

To begin with one aspect of this issue, the problem of monotony. Monotony in work, arising from the subdivision and mechanization of labour and the stereotyping of operations, has been increasingly a characteristic of industrialism. It is widely believed by the workers that the analysis and re-synthesis of operations by motion study and time study will inevitably increase this tendency by further crystallizing the details; and that although energy may thus be saved, the increase in monotony will outweigh that economy in dynamic value. That may or may not be true. The psychologist usually makes the rejoinder that the monotony is there in any case, that the workers will fall into some stereotyped habit or other, and that it is better that that habit should be an intelligent one, based on science rather than on accidental circumstance. Such a reply appears to be sound argument, provided one takes the monotony to be inevitable. And it is extremely difficult to see how it can be obviated for the great mass of workers in large-scale industry. Yet it appears to me that in falling back on its inevitability we are rather shirking the psychological issues. It might conceivably turn out to be true that on psychological grounds the far-reaching effects of continued monotonous occupation were so dangerous to members of a highly complex civilisation as to make it worth while to re-organize our methods of production root and branch. I do not suggest that this is so, but only that it might be so; and that it is for the psychologist to develop its full implications. Münsterberg[1] meets the problem by showing that there are those who do object, and those who do not object to long-continued monotony in work. But that is a statement most provocative of further questions. Are those who find nothing irksome in repetitive work sufficiently numerous that with proper methods of selection there will be enough to go round all the monotonous tasks? More important, is that difference in response to monotony a simple innate difference? Or a complex psychological symptom determined by other more general tendencies and qualities of mind and character? If the latter, does it indicate an inferior or a superior type of personality? One that will respond adequately to the complex calls of a democratic community, which needs intelligence and responsibility among its members, or one that will be a dead-weight, a focus of indifference and even of evil? How far is that indifference to monotony a product of a particular type of education? And how far is that education one which we praise or condemn on other and wider grounds? How do those who do not object to monotony occupy their leisure? What contributions do they make to the moral and intellectual life of the community?

Another point made by some psychologists relative to this issue is that repetitive work can be carried out automatically, allowing attention to be given to thinking out other problems. To a greater or less extent, this is, of course, true. But is it a desirable thing? Is the condition of automatism and dissociation one that it is wise to induce for more than a very short period per day? It is, I suppose, true that small doses of it will harm no one and it may even give welcome relief from the strain of concentrated effort. But is it safe to build a complex social system upon such industrial conditions? What effect does such habitual dissociation for, say, eight hours per day produce upon other social reactions and upon the intellectual and emotional life of the worker? What indications we have are not in favour of this condition. Mr Wallas[2] records a case of one of his students throwing up a good job at Woolwich solely on grounds of the intolerable monotony. One of my own students, a man of admirable qualities, bearing his part well in all relations, a thinker, a steady self-controlled man, and moreover, one whose workmanly impulses gain much satisfaction in activities outside his office, has confessed how the monotony of his work in the Post Office has had, as he puts it, a "terrible effect" upon him, making him "feel he must shout or punch somebody"! How much more serious and insidious must that effect be upon less stable personalities? These and many other questions fraught with meaning for the welfare of our civilisation, no one can yet fully answer. But upon this answer depends our final judgment as to the significance of monotony in work.

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[1] H. Münsterberg, *Psychology and Industrial Efficiency*, London, 1913.

[2] Graham Wallas, *The Great Society*, London, 1914.

(*d*) Closely connected with the problem of monotony is that of loss of craftsmanship. There is a fear, widely held and clearly expressed, that time study, motion study and managerial methods based on these will, by the further sub-division and stereotyping of tasks, of necessity increase the inherent tendency of large-scale industry to destroy individual craftsmanship. To the expression of this fear one replies that the whole point of the analysis of operations is to increase skill and understanding, and that with this end in view the psychologists are the first to emphasize the need for the selection and training of workers. That reply, however, is met by the objection that it is a *'task,'*—a *job skill*—that is thus reached, not a *craft skill*; that, in fact, such analysis of itself leads to the greater specialisation and narrowing of operations. The selection and training of the workers, it is said, themselves lead further in this direction, when built upon time study and motion study. The point at issue, moreover, is not the desirability of skill and knowledge, but the question of the persons in whom that knowledge shall be deposited. It is a question of who shall be responsible for the planning of operations and the details of method. The workers fear that the scientific knowledge which underlies the choice of method on the new lines will in the end be divorced from the manipulative skill itself, and will become the possession of the management and the management alone. If to this one says that psychologists are stressing the need for the understanding by the workers of the modes of determination of time and motion, for the intelligent co-operation of the workman, and even for the opportunity and encouragement to improve and devise further methods themselves, the reply is made that this comparative freedom and knowledgeable co-operation appear only to be possible in the early stages of the application of these methods; that the full logical development of time study and motion study, of the control of rhythm and rest, is towards the complete specialisation of the knowledge on which the last details of repetitive movements are based. The mind of industry is then no longer to be found in office and workshop alike, as regards differing problems, but is concentrated entirely and in all respects within the office. The picture drawn by Mr Taylor of the group of pig-iron handlers, who, in his words, "more nearly resembled in their mental make-up the ox than any other type[1]," and who found their minds and wills lodged in the person of the man who held the stop watch, this picture is taken to represent the essentials of the end state of the industrial developments which are set going by psychologists.

It is to be realised that in these fears, English student workers are greatly influenced by their acquaintance with what actually has happened under American systems of 'Scientific Management,' and by the formulated objections of American Trade Unionists on these points. The considered opinion of organized labour there is that time and motion studies, combined with functional foremanship (which is itself a further instance of high specialisation), tend to "gather up and transfer to the management all the traditional knowledge, the judgment and the skill, and monopolizes the initiative of the worker in connection with the work; that it intensifies the modern tendency towards extreme specialisation of the worker and the task; that it splits the work up into a series of minute tasks, and tends to confine the worker to the continuous performance of one of these tasks, thus eliminating skilled craft, and depriving the worker of thought, initiative, sense of achievement and joy in his work[2]." Such a strongly worded summing-up of the experience of labour under particular systems cannot fail to have profound influence upon the attitude of English workers. Mr Taylor, himself, has, of course, openly avowed the purpose of transferring knowledge to the management, and when one urges that English psychologists are facing these problems, not with rigid fully developed 'systems,' but with the spirit and intent of full inquiry and research into all implications of their method, and that these evils may be by no means inevitable, one is met again by the expressed conviction that, once initiated, these processes will move on by their own momentum to the full logical development of extreme specialisation and mechanization of the workman. The workers inquire what safeguards against these essential tendencies we are able to devise. One that has been suggested is that no workman shall be kept for an indefinite period on one single task or narrow range of operations, but shall be shifted on to other tasks at periods, in order to develop adaptability and maintain interest and variety in work. It is replied that the economic interests involved are such that this shifting of the workman for reasons that are economically inadequate could never be more than occasional and sporadic. The reasons for such shifting are long-sighted reasons, and the practice would probably exert no immediate beneficial effect upon production. When a worker has by special selection and careful training been fitted for a particular task, to turn him on to another,—unless it were so closely allied that the change would have no value for our argument,—would rather involve an immediate and distinct loss to the employer, as well as multiplying records and rendering wage-accounting more complex. Employers being human tend to be short-sighted; and although it might be shown that in the long run the maintenance of interest and adaptability increased rather than lessened productive capacity, yet so long as the mere purpose of immediate production is over-emphasized by industrial philosophy, the employer has every encouragement to neglect the long-distance factors, to say nothing of the wide social effects of his practices. Thus, the workers say, there seems to be no way out of the dehumanising tendencies of the methods under discussion, in spite of their face value emphasis on the need for training and the reward of skill.

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[1] F. W. Taylor, *Principles of Scientific Management*, New York, 1911.

[2] R. F. Hoxie, *Scientific Management and Labour*, New York, 1915.

It will however be argued against these difficulties of the workers, and of course with every degree of justice, that the scientific study of industrial operations does not create the specialising tendencies of industry. Those are inevitable in any case. The real problem is no longer whether it is possible to return to mediaeval craftsmanship, but the detailed problem of how far and in what manner we can reap the fullest advantage of modern machinery, while avoiding its evils. And this full human control of machinery for human ends can only be gained when the science of the relation between man and machine is fully developed. We can only control what we understand; and it has been the blind wastes and inefficiencies of the past that have given rise to most of the evils that the workers deplore. I have said this to the workers in conference; and their reply brings us to the very heart of the matter. That reply is,—whether our science is able to serve the greater human ends depends entirely on how far we keep those greater ends in view. Science herself is impartial, and lends herself as easily to destruction as to construction.

(*e*) The workers ask, then, what are the ends which we are serving? When we speak of Production, they ask, "Production of what?" "Production of things or of men? Of goods or of human well-being and happiness?" It has been said to me over and over again, "There are things more important than mere production, and one of these is human personality." The criticism by these educated men of our emphasis on production is not on the fallacious ground of 'over-production,'—a fallacy they understand as well as ourselves; it is on moral and social grounds. They over-ride the artificial barriers which the sophisticated erect between economic, psychological and ethical questions, and ask that we shall view industrial processes in their proper relation to the full needs of human nature. They have even pointed out to me that our science is incomplete unless it deals with the wide social effects of technical processes. They do not deny the need for production, but demand some social guidance of that purpose in relation to moral ends.

Moreover, they are seeking to find in their daily occupation a true vocation,—one which shall develop them further in their manhood and employ the balanced powers of mind and body. It was asked on one occasion, "Is a man in industry sleeping or living? Is he just as in bed, marking time, existing, but not living? Or is he really living in the full human sense?" When one speaks to them of 'vocational tests,' one meets sometimes with derision. We wish to test, they say, not for a vocation but for a mechanical operation; and the term 'vocation' is meaningless in such a connection.

In response to this demand, one is able to urge that we are asking for a wider education, general and technical, in order that each worker may come to understand the part he plays in relation to the complete scheme of industrial activity. The workers admit that such an imaginative understanding of the relation of each bit of work to the great creative whole of industry is a most desirable thing, but they go on to urge that this cannot of itself satisfy the desire for creative work. It will rather aggravate the emotional dissatisfaction, unless there goes with it some measure of effective control of the industrial machine. If the worker himself and the part he plays in the industrial whole are directed entirely from without, the mere knowledge of how he is directed can be but an exasperation of his feelings of impotence and futility. Therefore, he argues, the assumption by the worker of some measure of genuine control of industrial processes is the only way in which it is possible to restore to the vast dehumanised machine of modern production any true satisfaction for the workmanly and creative impulses of the bulk of those whose destiny it controls. This is their answer to the problem of over-specialisation, to the question of how the technical psychology of industrial processes can be made to serve the greater human purposes.

IV.

Here, then, in the course of our inquiry, we are driven upon a controversial question of the first magnitude, one of those points in which psychology and ethics and economics are inextricably interwoven. The exact form or degree of control which can or should be given is not germane to our present discussion, and presents a problem bristling with difficulties of every order. I am concerned here only with the broader psychological aspects of the actual point of view of the workers, and shall not attempt to enter the controversy. It appears to me however very necessary that we should realise how much this demand for control is a psychological question, and how inevitably it grows out of the actual psychological conditions of the world as it is to-day. Even psychologists are not unknown to deal with this problem by labelling those who make the demand as mere 'agitators,' or at best as 'idealists'; but that is again a simple shirking of the issues. It is our business to inquire what conditions lie behind the demand, what are the psychological influences which have led to its formation. My own reading of the situation is that this conscious self-assertion and the desire for self-government in industry are the inevitable outcome of the growth of political democracy, of the complex demands that civic and national life is now making on the personality of all its members, and of the wider and freer education that recent years have brought to many. It is not so much a matter of the spread of this or that particular social philosophy. That is indeed an influential factor, but is probably itself but a conscious expression of deeper-lying actual tendencies. It is rather the actual calls upon individual and moral judgment, upon personal responsibility in the many-sided activities of our present-day social life, that are reacting upon that part of social life whose main function is the supply of material necessities.

Social psychologists have made clear what onus is thrown upon the individual moral judgment in modern ethical relations, and how increasingly the centre of moral gravity shifts from the external authority to personal responsibility. They have stressed the significance of the 'self-regarding sentiment' as the core of that type of character best adapted to the complex relations of our society. Far as the majority of people are from the highest type of character, the actual tendencies of moral development are all in this direction. Adequate response to political relations in civic, in national and international concerns shows the same call for individual judgment and responsible knowledge.

Students of political life, of electioneering methods, of the 'stunt' press, of the crowd spirit, of all the evil and dangerous aspects of democracy, stress again and again the need for a further development of personal intelligence and responsibility.

Pathological psychology joins its voice with social psychology in deprecating suggestion as a social method, and suggestibility as an individual condition. Educational psychology indicates that the way to avoid that condition is to call forth self-control, to give occasion for moral choice, even in the earliest years. The dynamic centre of education is now the individual, and the keenest minds are given to the problem of 'education for freedom.' This again is itself but an indication of the imperative call of the 'Great Society' for individual intelligence and responsibility. Analytic psychology, moreover, is making clear the dependence of personal and social health upon the satisfaction, in some degree and in some form, of the self-feelings; repression of the higher forms leading to their attachment to the lower. The simple doctrines of earlier social philosophers as to the essential egotism of human nature are now thrown into their proper expression; and the crude insistence on the instinct of self-preservation by those who base Society upon the 'economic motive' is now given its true psychological value. Self-fulfilment is shown to be a prime condition of mental and moral stability, for individuals and for social groups. There is, further, an increasing mass of evidence to show that this self-fulfilment is best achieved through creative activities in 'work.' It is interesting to students of social philosophy to observe how greatly the essential doctrine of Ruskin and Morris,—the doctrine of salvation through creative work in everyday life,—is being supported by so much in recent psychology. Whether one considers the psychology of little children as made clear by Madame Montessori's experiments, the analysis of the crimes and misdemeanours of adolescence, the emphasis now laid, for psychological reasons, upon constructive handiwork and art as an essential method of education,—everywhere one finds a stressing of self-expression through work in relation to everyday realities.

A similar weight is given to it by many physicians with regard to the re-education of the neurasthenic. In the words of Captain Brock, "Work is the one indispensable channel through which life has to be awakened in the young, or rendered back to the devitalized[1]"; but the work meant is creative activity which affords a real expression for the self, mechanical or routine occupation leading only to further dissociation.

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[1] Capt. A. J. Brock, M.D., quoted in *Cambridge Magazine*, August 2nd, 1919.

V.

It is in the light of these widely garnered facts that the problems of monotony, of mechanization and specialisation in industry, and of the possibility of some kind of self-government by the workers appear so pregnant with meaning for the welfare of our civilisation. And it is for these reasons that we are impelled to view the problems of industry in their larger relations. One can see two diverging tendencies in modern life,—one is the increasing emphasis on salvation through work, more and more justified by psychological knowledge, the doctrine of 'vocation.' The other, the doctrine of 'leisure,' stresses the right use of the hours left over when work is done; a doctrine increasingly necessitated by the mechanical development of the material basis of life. The issue between these two is no academic one, but one most pertinent to the health and therefore to the permanence of industrial Society. It bears the closest relation to the detailed problems of the industrial psychologist, as, *e.g.*, the question of hours, and to the direction in which his methods shall be developed. And again we must say that it is at bottom a psychological question, one of the actual functioning of the human mind. Are the mechanical necessities of industry such as to allow, under conditions we can devise, the bulk of workers to find a vocation, a full healthy satisfaction for the creative impulses and self-feelings? If not, is it actually possible to have specialisation and mechanization within the 'Great Industry,' and response to the varied and complex calls of the 'Great Society' outside it? Is it psychologically possible to have docile, externally controlled workers in industry, who are yet free, intelligent and responsible members of a democracy outside it?

These are psychological issues we shall be driven to face sooner or later. And they are the actual issues presented to us by the thoughtful among the workers themselves. Everyone of these arguments has been put to me, often of course in crude and simple words, by student workers. They are not only able to articulate their own instinctive desires, but are also aware of the wide tendencies in education and even in psychology. As students of politics and social conditions, familiar with the evils and dangers of modern democracy, and with the efforts of education to create the right responses to social calls, they approach these problems of industry with the most intimate and earnest concern for their solution. But what they articulate, and are able to interpret in a scholarly form in relation to science and philosophy, is after all inherent in the attitude of the dumb driven mass, restless and dissatisfied, able to express its dissatisfaction only in terms of hours and wages. Education is often naïvely appealed to, and even by psychologists, as a panacea for 'industrial unrest.' The whole of history is an answer to such a view. Education is a safeguard against blind revolution; it does prevent the outbreak of merely destructive forces. But a wide and liberal education, such as will fit a citizen for his citizenship and a member of a great modern community for his part therein, will essentially militate against a routine function in industry, and will increase the strength and pertinence of the criticisms which the workers bring against monotonous over-regulated industrial life. This is again surely obvious on psychological grounds. It is known to be true by all those who have experience in the higher education of working people. It is just those who have read the widest and thought the deepest in literature, history, psychology, and I may forcibly add, in economics, who raise the most fundamental difficulties on the issues we have discussed.

It is then because I have found these arguments among the educated workers and felt the difficulty of answering them, that I have wished to develop the analysis of the worker's attitude. If we are to gain the cooperation of the most influential groups, whose goodwill would count most heavily in our task of psychologising industry, we must appeal on the broadest issues. Until we face, and show clearly that we are facing these larger, more vital problems, we shall not gain their full confidence in the smaller. Yet it is not only a matter of gaining the goodwill of the workers. It is that we do owe service to these larger purposes ourselves. The workers come to Psychology as to the human science, the science which, whatever else be prostituted to meaner ends, will of its essence consider the whole man, in all his relations. It is for us more than for any other science to lend our knowledge for the re-creation, not only of industry, but of human society. To do this we must see the lesser in relation to the greater, and keep our vision whole.

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SUGGESTION AND SUGGESTIBILITY[1].

By E. PRIDEAUX.

§ 1. *Definitions of suggestion.*
§ 2. *Varieties of suggestibility.*
§ 3. *Classification of the responses to suggestion.*
§ 4. *Abnormal suggestion.*
§ 5. *Suggestion as a method of treatment.*

§ 1. Definitions of Suggestion.

The controversies between psychologists and neurologists as to the nature and treatment of the psycho-neuroses are largely due to the employment by both parties of the same words in different senses, and as a striking instance of this, the word 'suggestion' has been responsible for considerable confusion. For many neurologists suggestion is the beginning and the end of all diseases of psychogenetic origin, both as an aetiological factor and as a method of treatment. Even amongst psychologists there seems to be no real agreement as to the meaning of the word, and it is often used as if it were an explanation for a mechanism which is not understood.

The various definitions which have been given of the term 'suggestion' make it obvious that different degrees of the same process are being referred to. These definitions can be divided roughly into two classes according as they refer to (*a*) *Normal Suggestion*, which takes place in every-day life in all of us, or (*b*) *Abnormal Suggestion*, which takes place in psychoneurotic patients, and in normal persons under abnormal conditions. It is difficult to draw any hard and fast line between the two for the difference is only one of degree, and appears to depend on the individual tendencies of the subject, whereas the mechanism remains the same in both classes. The term 'suggestion' as used by Janet, Dejerine, Grasset, and Babinski refers to abnormal suggestion only. Janet defines it as "the complete and automatic development of an idea which takes place outside the will and personal perception of the subject[1]," and Dejerine and Grasset hold a similar view. For Babinski the process is one of suggestion only when the idea conveyed is unreasonable[2]. The broadest definition is that of Bernheim, who defines it as "the process by which an idea is awakened in the mind of a subject and accepted[3]." This definition includes all varieties of suggestion, but does not clearly mark it off from other mental processes. The definition which is now often accepted by the English School of Psychologists is that of Dr McDougall given in his *Social Psychology*, *viz.*: "Suggestion is a process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate grounds for its acceptance[4]." This definition includes both normal and abnormal suggestion, but does not make it clear whether the action is limited to processes in which there is a relationship between two persons only, a limitation which seems to be unnecessary, as it excludes auto-suggestion. Dr McDougall could improve his definition and make it include every variety by making it read "suggestion is a mental process resulting in the acceptance with conviction of a proposition in the absence of logically adequate grounds for its acceptance[5]," and this is the definition which I put forward for the purposes of this discussion. Dr McDougall classifies suggestion amongst his general innate tendencies as a pseudo-instinct[6], and I think it would be profitable to discuss whether it is necessary to maintain this view or whether the process of suggestion cannot be explained in some other way.

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[1] Read before the Medical Section of the British Psychological Society, 29 October, 1919.

We have then to explain why it is that we accept with conviction and act upon propositions made or occurring to us without any adequately logical grounds for so doing.

If there are logical grounds for accepting the proposition the idea is generally called *persuasion*, but it is difficult to separate this from suggestion, and it seems better to include it, at any rate when used in psychotherapy, as a form of normal suggestion, for there are often no logical grounds for accepting the proposition, but only logical grounds on the part of the physician for persuading the patient to accept it; moreover to obtain conviction affective processes must come into play, for the way of saying a thing is more important than what is said, which is expressed in the statement "Manner is more important than matter."

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[1] Janet. *Mental State of Hystericals*, 240.

[2] Quoted by Bernheim in *Automatisme et Suggestion*, Alcan, 1917, 55.

[3] Bernheim. *Hypnotisme, Suggestion, Psychothérapie*, 2e éd. 1903, 24.

[4] W. McDougall. *An Introduction to Social Psychology*, 12th ed. 1917, 95.

[5] Dr C. S. Myers in a communication to the discussion asks, "Do not inadequate logical grounds often act suggestively, *i.e.* induce a conviction far above their intrinsic merit?" He suggests a further improvement by substituting "apart from the intellectual outcome of pure judgment based on logical premises" for "in the absence of logically adequate grounds for its acceptance." With this I fully agree and would like to accept this improvement, for it at once removes the difficulty of separating persuasion from suggestion.

[6] *Op. cit.* 90.

There can be no doubt that *suggestibility* is the chief factor in the process of suggestion, and that the process is a subjective one; we have learnt as the result of psycho-analytical investigation that this state is not a passive state of receptivity, and that the mind cannot be compared to a vacant seat waiting for someone to fill it, as was originally held, but that it is the result of active mental processes going on in the mind of the subject and particularly of affective processes.

Dr Ernest Jones has called attention to the distinction between verbal suggestion on the one hand and affective suggestion on the other[1], and maintains that the latter is the more fundamental, and is the necessary basis for the former, which view accords with Bleuler's statement, quoted by Dr Jones, "Suggestion is an affective process[2]." This view seems to be by no means generally accepted, but a consideration of the facts with which we are familiar concerning suggestibility compels us to admit its truth, and an examination of those facts shows more clearly the general nature of the whole process of suggestion.

§ 2. VARIETIES OF SUGGESTIBILITY.

The chief facts are that (*a*) Suggestibility varies in different persons irrespective of the nature of the suggestion, and of the suggestor, (*b*) Suggestibility varies in the same person at different times and under different conditions, (*c*) Suggestibility may have reference to a particular system of ideas only, (*d*) A person may be suggestible towards one person and not towards another. I call these four distinct states of suggestibility: (*a*) Individual, (*b*) Conditional, (*c*) Specific, (*d*) Personal.

(*a*) *Individual Suggestibility.* The fact that suggestibility varies in different persons irrespective of the nature of the suggestion makes it important in psycho-therapy to be able to recognise what other characteristics are associated with exaggerated suggestibility. It is exaggerated in the child and diminished in old age; it is exaggerated in those whose egoistic instinctive tendencies are excessively developed and who make little attempt at self-control and so act on impulse, the class of persons originally described as having a sanguine temperament. This class corresponds to the 'extrovert' of Jung, the 'motor' type of Baldwin, the 'objective' type of Bain, and the 'tough-minded' of James. Suggestibility is also exaggerated in crowds whose other characteristics are impulsiveness and incapacity to reason with absence of judgment and of the critical spirit. Le Bon points out that "the decisions affecting matters of general interest come to by an assembly of men of distinction, but specialists in different walks of life, are not sensibly superior to the decisions that would be adopted by a gathering of imbeciles[1]."

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[1] *Papers on Psycho-analysis*, 2nd ed. 1918, 319.

[2] *Op. cit.* 320.

I think also that suggestibility is more marked in those who live in the South and warm climates than in those who live in the North and cold climates, and that those, whose associations of ideas take place by contiguity, are more suggestible than those who associate by similarity.

It is less marked in those who hold strong principles and ideals, in methodical thinkers, whose critical powers have been well developed, and in those with the so-called 'bilious' temperament, who correspond to the 'introvert,' the 'sensory' type, the 'subjective' type, and the 'tender-minded.' Suggestibility is very much exaggerated in the patient with 'conversion hysteria' and this has led Babinski to enunciate his conception that hysteria is due to suggestion. This conception has been accepted by many neurologists in this country, who have little knowledge of the mental processes at work in the process of suggestion and use the term in a very limited sense. When we recognise suggestion as an affective process, then we can agree with Babinski that hysterical symptoms are produced by suggestion, but we shall not be able to accept his view that hysteria be limited to the symptoms of 'conversion hysteria.' I therefore think it is unfortunate that Dr Rivers should have proposed the use of the term 'suggestion neurosis' as a substitute for 'conversion hysteria' in his paper "War Neurosis and Military Training[2]." Moreover so long as the present confusion exists in the meaning of the term suggestion the less we use it the better.

Investigations which I have been carrying out during the past year on the 'psycho-galvanic reflex' point to the fact that exaggerated suggestibility is always associated with a low 'emotive response,' and Dr Snowden informs me that similar results have been obtained by Dr Golla and himself at the Maudsley Hospital. If it could be shown that the converse is true, that a low emotive response is always associated with exaggerated suggestibility, then we should have a means of measuring suggestibility. More work needs to be done on this subject, for apart from the fact that we are not yet decided as to the physiological nature of the reflex, it seems certain from the psychological standpoint that two factors must be taken into account, the liberation of emotion on the one hand, and the stimulation of 'contrary' forces on the other.

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[1] *The Crowd*, 11th impression, 1917, 32.

[2] *Mental Hygiene*, II, 519.

I use the term 'contrary' forces in order to avoid the words 'repressing,' 'inhibiting,' and 'controlling.' I mean the forces which are brought into action by stimulation of mental processes on a higher level: they are 'contrary' as applied to suggestion, and act in opposition to the instinctive processes on the perceptual level. Physiologically they are the forces set free by stimulation of the cerebral cortex; following Dr Rivers in the symposium on 'Instinct and the Unconscious[1],' I might perhaps use Dr Head's term and call them 'epicritic.' It is possible that the psychogalvanic reflex may be an indication of the strength of the 'contrary' or 'epicritic' forces stimulated by the liberated emotion, and that it is not merely an emotive response.

From a consideration of these facts we can explain individual suggestibility as being due to the varying degree in which the egoistic instinctive tendencies are developed and the manner in which the sentiments have become organized to form ideals and act as contrary forces.

(*b*) *Conditional Suggestibility.* The variation of suggestibility in the same person at different times and under different conditions seems to depend upon the affective state in which the person happens to be, and the relation of the suggested idea to that state. I have found that even patients, who generally go into a deep state of hypnosis, are resistant to hypnosis on one day and will go off into their usual state on the next. Suggestibility is increased during hypnosis, fatigue, illness and prolonged emotional states, and by the effect of alcohol and certain drugs, conditions in which the 'contrary' forces are weakened. A wife, for example, recognises that a husband is more suggestible after a good dinner and chooses this time to get her propositions accepted.

(*c*) *Specific Suggestibility.* That suggestibility may refer to a particular system of ideas only is also an important fact pointing to the affective nature of the process. A person is specially suggestible to ideas that are pleasing to him and which satisfy his egoistic instinctive tendencies; according as those specific tendencies are developed so does his suggestibility vary towards ideas which evoke them, and we recognise that in the same family these tendencies are developed in each of the children in varying degree.

With the growth of sentiments and the appearance of complexes and interests, both actual and dispositional, as the result of experience, so does the suggestibility vary according as the suggestions harmonize with the affective states induced by them. Thus each person has his own particular sphere of suggestibility, and even under hypnosis the suggestibility is not the same for all suggestions. This is more clearly explained by a quotation from a paper by Dr Jones on "Psycho-analysis and Education," "A desire that arises in a person's mind for the first time is not likely to be very effective or significant unless it becomes attached to others that are already present; in other words a motive appeals more readily to him if it is linked, by resemblance, to earlier ones that are already operative in him[1]."

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[1] This *Journal*, 1919, x. 4.

(*d*) *Personal Suggestibility.* Suggestibility towards one person and not towards another depends on the affective processes operating between the two persons. Sympathy, respect, and confidence between the subject and the suggestor favour suggestibility. I have found it more difficult to hypnotize the patient of a colleague than one of my own.

Anything which tends to increase the authority and prestige, either personal or acquired, of the suggestor, increases suggestibility in the subject; thus a parent can produce suggestibility in a child, a teacher in a pupil, and a physician in a patient.

Dr McDougall has pointed out that the personality comes into play in virtue of the relative strengths of the two instincts of 'self-assertion' and 'subjection.' "Personal contact with any of our fellows seems regularly to bring one or other, or both of these instincts into play[2]," so that suggestibility is only evoked in us by persons who make upon us an impression of superiority of any kind in the particular situation of the moment. Dr McDougall relies on the strength of these two instincts to explain individual suggestibility; but although they are important—especially the instinct of self-assertion in virtue of the part it plays in the organization of the sentiments on a higher level—other instincts play an equally important part. It is probable that prestige owes its power to the complex emotions of admiration and awe, and often of gratitude and reverence, which are evoked by the instincts of curiosity, subjection, self-preservation, and the parental instinct. Whether we accept Freud's view, that the above tendencies are but sublimations of the sexual instinct, or not, we are bound to admit the influence of the sexual instinct, for we know that a sentiment of love or affection favours the sympathetic induction of emotion between two persons. According to Ferenczi: "Everything points to the conclusion that an unconscious sexual element is at the basis of every sympathetic emotion, and that when two people meet, whether of the same or the opposite sex, the unconscious always makes an effort towards transference[1]," and that this transference has its deepest roots in the repressed parental complexes. It is also significant that in the two classes of homo-sexuals, described by Ferenczi[2], the 'subject' and 'object homo-erotics,' the 'subject-homo-erotic' has an increased suggestibility, and although my experience of these cases is small, it is that the 'object-homo-erotic' is not at all amenable to suggestion.

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[1] *Op. cit.* 583.

[2] *Op. cit.* 99.

It is outside the scope of this discussion to go further into the psycho-analytical standpoint, and Dr Jones has already set forth the Freudian point of view in his paper on the "Action of Suggestion in Psychotherapy[3]," to the effect that 'suggestion' is a special variety of transference, namely, that concerned with the transference of positive affects to the physician, and that suggestibility takes its root in the masochistic component of the sexual instinct. It is impossible for anyone to discover the truth of Freud's theories without psycho-analytical investigation: my own position is that I accept the greater part of Freud's theories in so far as the fate of the 'pleasure-principle' is concerned, and his theory of sexual development and sublimation has been confirmed by my experience in psycho-analysis, but I think that the development and sensitivity of the 'reality principle' is of much more importance than Freud seems to allow and that Janet is right in so far as he emphasizes its significance.

A consideration then of facts shows that all four forms of suggestibility, which I have described as individual, conditional, specific, and personal, come into play in the process of suggestion, and that these are affective states evoked by the stimulation of different instinctive tendencies, sentiments, interests and complexes.

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[1] Ferenczi. *Contributions to Psycho-analysis*, 1916, 55.

[2] *Op. cit.* 253. Ferenczi uses the word 'homo-erotism' as being preferable to the ambiguous expression 'homosexuality,' since it makes prominent the psychical aspect of the impulse in contradistinction to the biological term 'sexuality.' He holds that of the two types of homosexuality, the passive form alone is one of true inversion, when homo-erotism occurs through subject-inversion, and therefore he calls this type the 'subject-homo-erotic.' In the 'active homosexual' the object alone is exchanged, and so Ferenczi refers to this type as the 'object-homo-erotic'; he regards this latter form of homo-erotism as being an obsessional neurosis.

[3] *Op. cit.* 318 *et seq.*

§ 3. CLASSIFICATION OF THE RESPONSES TO SUGGESTION.

Any explanation of suggestion must explain not only why suggestions are accepted, but also the circumstances under which they are refused, and even strongly opposed. If we take the results of attempts at suggestion in everyday life we can classify them into three groups:

(*a*) Positive response when the suggestion is accepted.

(*b*) Negative response when the suggestion is opposed.

(*c*) Neutral response when the suggestion is refused.

These results depend on the relationship of the suggested idea to the different states of suggestibility already described.

(*a*) *Positive Response* may be immediate or delayed; the immediate response gives us the most typical example of the process of suggestion, for in the delayed response there are also at work other factors which I shall describe under 'neutral response.'

Most writers are inclined to the view that if a suggestion is accepted it is due to the inhibition of other ideas opposing its acceptance and that realisation of the idea takes place simply by ideo-motor action. This view involves the difficulty that it depends on the meaning of inhibition, and that we do not understand the nature of ideo-motor action.

Dr Hart, in his paper "Methods of Psychotherapy[1]," attaches great importance to inhibition, but recognises that it is brought about by affective processes; for him 'suggestion' is 'complex thinking,' by which he means thinking due to the action of a complex, using the term complex in a very wide sense. He speaks of the capacity of suggestion 'for inhibiting conflicting ideas and tendencies.' This seems to be an inadmissible use of the word 'inhibition': the verb 'to inhibit' is an active and transitive verb, and the word 'inhibition' thus conveys the idea of an active process. If I go downstairs, it might be true to say that by so doing I was inhibited from going upstairs, but it would hardly be a correct usage of the word; the idea of going up would never arise and would not require inhibition. Suggestion has no capacity for inhibiting ideas, but, if we speak in terms of inhibition, is rather the consequence of the inhibition of inhibiting forces normally involved in volition.

Moreover, the term 'complex thinking' lays too much stress on the cognitive aspect of the process of suggestion, and though this is the first stage in the process, more than this seems to be involved; for example, if, during a railway strike I merely thought of an engine as the means of transport for getting me to town, nothing further would result, but if forces were aroused in me sufficiently strong to make me 'tip' the engine-driver, then I should be acting under the influence of suggestion. No other ideas would arise if the response was immediate, and no ideas would be inhibited.

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[1] *Proc. R. Soc. of Med.* March, 1919.

The term 'ideo-motor action' is a relic of the old psychology of ideas; for example, for Hegel, "an idea is a force and is only inactive in so far as it is held in check by other ideas[1]." If the process of ideo-motor action be analysed it is found that the action depends entirely on the affective forces aroused by the idea and that no idea will realise itself, unless it is reinforced by some affective force. Ideo-motor action is thus equivalent to the expression of emotion. We know that emotion is expressed normally through the autonomic nervous system, and when excessive, through the central nervous system[2]. It is evident that persons, whose instinctive tendencies are highly developed, and whose sentiments have not been well organized to act as 'contrary forces,' will realise their ideas in action through the central nervous system without opposition, and I have shown elsewhere that this may be an explanation why a patient with conversion hysteria develops symptoms attributable to the central nervous system and a patient with anxiety hysteria develops symptoms attributable to the autonomic nervous system.

It follows from what I have already said that I here maintain the view that an idea is accepted because it harmonizes with some preformed interest, sentiment, or complex, that the affective forces involved give it the necessary reinforcing power to realise itself in opposition to all 'contrary forces,' and that it is these affective forces which produce conviction. Any condition which tends to weaken the 'contrary forces' on the one hand or strengthen the compatible affective forces on the other favours the process of suggestion, and we have noted that the conditions which cause 'conditional suggestibility' are those which weaken the volitional forces, and that individual suggestibility is exaggerated in those who are endowed with strong emotional tendencies and have a poor development of self-control.

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[1] Hegel's *Philosophy of Mind*, 1894, 167.

[2] That emotions gain expression through discharges along the autonomic nervous system has been shown by the work of Pawlov, Cannon, Elliott and others, who have demonstrated the connexion between emotion and the physiological reflex reactions of the glandular secretions. In healthy adult individuals emotion may be experienced without any expression through the muscles supplied by the central nervous system, though as the emotion increases it requires a distinct effort to prevent it from being so expressed; when the emotion becomes more intense the control breaks down and at first only the facial and voice muscles are affected; finally, if the emotion becomes excessive, the muscles of the limbs and trunk are brought into action.

Dr McDougall has shown that it is the organization and strength of the self-regarding sentiment in relation to the other sentiments, which determines our line of action and constitutes our self-control; as this higher control, though it relies for its strength on the self-regarding sentiment, involves the formation of ideals and is perhaps influenced by the herd instinct, I have called it elsewhere the 'social ideal self' as a contrast to the 'individual self[1].' Individual suggestibility then depends very largely on the strength of the social ideal self, and the weaker the social ideal self, the greater the number of complexes that remain unsublimated, and the greater are the states of specific and personal suggestibility.

Mr Trotter has pointed out how one form of suggestion, 'herd suggestion,' is due to the action of herd instinct, and that "Anything which dissociates a suggestion from the herd will tend to ensure such a suggestion being rejected[2]." It is owing to the influence of the herd instinct that we may accept propositions in regard to religion, politics, and education. Such beliefs are non-rational and are accepted by us as the result of accumulated suggestions.

The exaggerated suggestibility of children, occurring when they have reached the age of paying attention, which is in its turn dependent on the interest aroused, is due to the evocation of the instinct of submission, the weakness of the social ideal self and the absence of resistance complexes. For opposite reasons old people are less suggestible.

(*b*) *Negative Response* is the response obtained when not only is the suggested idea incompatible with pre-formed sentiments and interests, but it arouses contrary emotions and sentiments. This is the process which is called 'contra-suggestion.' The mechanism is the same as for the positive response, but an opposing set of forces are set in action with the production of a state of 'negativism,' a state which is the direct counterpart to suggestibility. This state like suggestibility may have individual, conditional, specific or personal tendencies. It seems to be a form of overdetermination due to the presence of antagonistic complexes, which more than counterbalance the forces of a weak social ideal self.

Like suggestibility, negativism may be exaggerated and become pathological; it is most marked in dementia praecox.

Some people appear to adopt 'negativism' as a habit; such are the people we call 'cranks.' I look upon the action of these people as being that of overdetermination, owing to the formation of complexes associated with painful experiences in the past.

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[1] Article on "Mechanism of Hysteria" in *Functional Nervous Disease*, 1920, IV.

[2] *Instincts of the Herd in Peace and War*, 3rd impression, 1917, 33.

In psycho-therapy, when we have established an atmosphere of cure, negativism signifies an unconscious resistance to recovery, and when exaggerated, it must make us suspect dementia praecox, or malingering if it seems likely that the resistance is a conscious one.

(c) *Neutral Response.* It is hard to draw the line between the lower forms of volition and suggestion. If the idea is incompatible with the social ideal self and the social ideal self is strong, then the process is one of volition and we get a neutral response: this is what happens in those who hold strong principles and ideals.

If the social ideal is too weak, and the affective forces aroused are strong, the idea is accepted and the process is one of suggestion.

If the idea has not made sufficient impression, we get a neutral response, and this is due to the fact that there are no pre-formed interests or complexes to which it can attach itself: in this case the suggestion is ignored. This is seen most markedly in certain imbeciles. The suggestion may need repetition to give it the necessary amount of prestige for acceptance, a fact which is taken into consideration as a basis for all advertisements. The suggestion may be incompatible with such interests and complexes as exist, and the forces involved simply neutralise each other, which is one of the reasons for the fact that the suggestions of the younger generation are not easily accepted by the elder.

A neutral response may also be the result of a conflict of motives due to the incompatibility of the interests aroused, with the production of a state of doubt, which is seen in an exaggerated form in cases of anxiety hysteria: in such cases the suggestion may be accepted after deliberation either to relieve tension, or when a decision is brought about by the reinforcement of one side of the conflict by further affective forces; or the conflict may be forgotten and only at some later period will one side of the conflict materialise by the stimulation of affective forces which harmonize with it. A neutral response will also occur in cases of 'dementia.'

§ 4. ABNORMAL SUGGESTION.

The process in abnormal suggestion is the same as in normal suggestion, and depends on the factors already discussed, which increase the various states of suggestibility. The difference is only one of degree, and in abnormal suggestion the affective forces are stronger, and the 'contrary forces' are weaker, so that the person is less or not at all aware of their action and has little or no control over them. This is best seen in persons suffering from hysteria; when their affective states are dominated by the desire to escape from some irksome duty or future danger, to astonish, or to attract attention or sympathy, any idea harmonizing with this state becomes reinforced and realises itself.

We also know that hysterical patients can temporarily put aside their hysteria by a change in their affective state when the situation requires particular concentration on some interesting function or amusement. I think that the disappearance of hysterical symptoms as if by magic in the presence of danger and the sudden recoveries reported in the newspapers are explained by the fact that one affective state is substituted for another by the re-direction of attention under the influence of surprise. I regard hypnosis, in accordance with Bernheim's view, as an exaggerated form of suggestion. I cannot accept Dr Rivers's statement in *War Neurosis and Military Training* that "In the hypnotic state the individual responds immediately and without question or hesitation, not merely to the command of his hypnotizer, but even to a desire or impulse of the hypnotizer's mind which is not expressed by speech or obvious gesture[1]."

It implies the presence of some mysterious force, an idea which we are only just beginning to uproot from the popular mind; my own experience and I think that of all other observers of recent times are entirely opposed to it.

The explanation of suggestion I have given seems to make it incorrect to classify suggestion as an innate tendency. We have seen that there is no one single state which we can call suggestibility, but that there are several states of suggestibility, and that these are induced by the stimulation of different instinctive tendencies, sentiments, interests, and complexes.

We have seen that a suggestion is accepted because it harmonizes with these states, and that the affective forces aroused give it the necessary reinforcing power to realise itself, and that it is these affective forces which produce conviction. We have now to consider briefly how we can apply this conception to suggestion as a method of treatment.

§ 5. SUGGESTION AS A METHOD OF TREATMENT.

The object to be attained in treatment by suggestion is to produce a condition of mind in the patient which will set in action the right affective forces for the induction of those states of suggestibility, which will harmonize with and reinforce the ideas to be suggested and so get them accepted with conviction.

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[1] *Op. cit.* 529.

The first step in the process is for the physician to induce a state of personal suggestibility in the patient, and this he does by arousing in him the necessary instinctive tendencies, showing him sympathy and impressing him with a knowledge of and interest in his condition, so that the patient has respect for and confidence in the physician. In neurasthenic hospitals, where an atmosphere of cure is present, this state is induced in him on admission by the patients, who have been already relieved of their symptoms, for success is the greatest creator of prestige.

The physician then proceeds according to the type of patient he has to deal with and to the amount of personal suggestibility already induced; if the states of individual and conditional suggestibility are exaggerated then it will not matter what form the suggestion takes; but if there is only specific suggestibility then it is necessary, if possible, to find out its nature by therapeutic conversations and superficial analysis and so to arrange the suggestions as to be compatible with it.

In one form of abnormal suggestion advantage is taken of the effects of some emotional reaction to alter the affective state. An emotion frequently used is that of surprise, which "tends to free the mind from what before occupied it, and to increase the intensity of every emotion with which it blends, or by which it is rapidly followed[1]." If the affective state be that dominated by fear it may be counter-balanced by inducing a state of anger for "the emotions of fear and anger tend to exclude one another from simultaneous activity[2]." I think also that painful electricity, the use of which has done considerable harm, and isolation, depend largely for their results on the change of affective states induced by them.

Other methods depend on the fact that the suggestion will not be accepted with conviction unless it is associated with some specific treatment to account for the cure. This form of suggestion is 'indirect suggestion' and is often used quite unconsciously by the medical profession. It is used either by insinuation with the help of massage, drugs, electricity, or by deliberate deception, for example, the use of water without morphia as an injection, and the use of bread pills for functional vomiting. Hypnosis is in most cases unnecessary, and although at one time I used it very extensively, I now only use it to clear up an extensive amnesia.

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[1] Shand, *Foundations of Character*, 1914, 422.

[2] Shand, *op. cit.* 260.

Of the methods of 'suggestion treatment' there is no question that the method of normal suggestion by explanation and appeals to feelings is the best as the patient then realises that he himself is responsible for the removal of symptoms and he will know what to do in case of a relapse. In the other methods he relies entirely on the physician, does not understand his condition, and is much more likely to relapse.

But in any case it must be noted that the patient relies on the explanation given to him, and that the real cause of the condition may never have been discovered. Treatment by suggestion does not therefore conform to our ideal method of treatment, and is not here advocated as such, but it is very useful in practice as a method of removing symptoms in certain cases.

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THE SINGLE GENERAL FACTOR IN DISSIMILAR MENTAL MEASUREMENTS.

By J. C. MAXWELL GARNETT.

NOTE: ***All the variables, whether dependent or independent, referred to below are distributed according to the normal law with the same standard deviation.***

I. Introduction.
II. Correlated variables expressible in terms of the same number of independent variables.
III. The number of independent 'conditions for a hierarchy.'
IV. The fact, that variables whose correlations satisfy the conditions for a hierarchy are vector compounds of a single general factor and specific factors only, may be obscured, as in Dr Thomson's dice-throwing experiments, when very many independent variables are used for the expression of the correlated variables in question.
V. The single general factor, and the specific factors, in Dr Thomson's experiments.
VI. Uniqueness of Professor Spearman's single general factor.
VII. Dr Thomson's results are consistent with the existence of Professor Spearman's single general factor in every set of sufficiently dissimilar mental qualities.
VIII. Summary of conclusions.

I.

The following paper is concerned with the relations between n correlated variables each of which is distributed according to the normal law, $y = e^{\frac{-x^2}{2\sigma^2}}$, with the same standard deviation, σ; and with the expression of such variables in terms of n or more independent variables each of which is distributed according to the normal law with the same standard deviation (σ) as before. The investigation confirms the result of a previous inquiry[1], namely that whenever the Bravais-Pearson coefficients of correlation between n variables satisfy the conditions for a hierarchy[1]—a condition which tends to be fulfilled by the $\frac{1}{2}n(n-1)$ correlations between pairs of any set of n sufficiently dissimilar mental tests[2]—the n variables in question can be expressed in terms of $n+1$ independent variables of which one is a single general factor, while the remaining n independent variables are entirely specific. In particular, it is shown that when the scores obtained in such dice-throwing tests as those described by Dr G. H. Thomson[3] are measured in new units such that their measures, already distributed according to the normal law, have the same standard deviation, these measures are variables which, when the number of dice is infinite, satisfy the conditions for a hierarchy, and when the number of dice is very large approximately do so; and it is further shown that these variables, although as Dr Thomson has pointed out they appear at first sight to possess no general factor but only group factors, may be expressed in terms of a single general factor with specific factors only.

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[1] Garnett, *Proc. Roy. Soc.* A, xcvi. 1919, 91 *et seq.*

II.

It is easy to show that n correlated variables $q_1, q_2, \ldots, q_n$ can always be expressed as linear functions of n independent variables $x_1, x_2, \ldots, x_n$ by means of equations of the form

$$q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + \ldots + {}_sl_n \cdot x_n \text{..................(1)}$$

where the l's are numerical coefficients such that

$${}_sl_1{}^2 + {}_sl_2{}^2 + \ldots + {}_sl_n{}^2 = 1. \text{ ....(2)}$$

Thus it has already been shown[4] that, if the whole number N of independent variables involved in all the q's be $y_1, y_2, \ldots, y_N$, the q's may be expressed in terms of the y's by means of the equations of the form

$$q_s = {}_sm_1 \cdot y_1 + {}_sm_2 \cdot y_2 + \ldots + {}_sm_N \cdot y_N \text{ ....(3)}$$

where the m's are numerical coefficients such that

$${}_sm_1{}^2 + {}_sm_2{}^2 + \ldots + {}_sm^2{}_N = 1. \text{ ....(4)}$$

Now choose new independent variables $x_1, x_2, \ldots, x_n$ given by equations of the type

$$x_s = {}_s\mu_1 \cdot y_1 + {}_s\mu_2 \cdot y_2 + \ldots + {}_s\mu_N \cdot y_N \quad [s = 1, 2, \ldots, n] \text{ ...(5)}$$

where $${}_s\mu_1{}^2 + {}_s\mu_2{}^2 + \ldots + {}_s\mu^2{}_N = 1 \text{ ....(6)}$$

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[1] Namely that, if r_{ab} denote the correlation between the ath and the bth of the n variables, $\frac{r_{as}}{r_{at}} = \frac{r_{bs}}{r_{bt}}$ where a, b, s, t have any different values from 1 to n inclusive. See footnote 2 on p. 244.

[2] Garnett, *loc. cit.* 105.

[3] This *Journal*, VIII. 1916, 271 *et seq.*

[4] Garnett, *loc. cit.* equation (8), 94. Cf. also Bravais, *Mémoires de l'Inst. de France*, IX. 1846, 260 *et seq.*

and where, since the x's are independent,

$$ {}_s\mu_1 \cdot {}_t\mu_1 + {}_s\mu_2 \cdot {}_t\mu_2 + \ldots + {}_s\mu_N \cdot {}_t\mu_N = 0 \quad [s, t = 1, 2, \ldots, n]; \ \ldots(7) $$

for the expression on the left-hand side of this equation is the coefficient of correlation[1] between x_s and x_t, and this is zero since x_s is independent of x_t. In order that the n q's may be completely expressible in terms of the n x's we must be able to write

$$ q_s = {}_sl_1 \cdot x_1 + {}_sl_2 \cdot x_2 + \ldots + {}_sl_n \cdot x_n \quad [s = 1, 2, \ldots, n] \ \ldots\ldots(1) $$

where, as before,

$$ {}_sl_1{}^2 + {}_sl_2{}^2 + \ldots + {}_sl_n{}^2 = 1. \ \ldots\ldots\ldots\ldots\ldots\ldots\ldots(2) $$

If now we substitute in equations (1) the expressions for the x's given in equations (5) and equate the coefficients of the respective y's to the coefficients of the corresponding y's in equations (3) we obtain

$$ \left.\begin{array}{l} {}_sl_1 \cdot {}_1\mu_1 + {}_sl_2 \cdot {}_2\mu_1 + \ldots + {}_sl_n \cdot {}_n\mu_1 \ = {}_sm_1 \\ {}_sl_1 \cdot {}_1\mu_2 + {}_sl_2 \cdot {}_2\mu_2 + \ldots + {}_sl_n \cdot {}_n\mu_2 \ = {}_sm_2 \\ \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ {}_sl_1 \cdot {}_1\mu_N + {}_sl_2 \cdot {}_2\mu_N + \ldots + {}_sl_n \cdot {}_n\mu_N = {}_sm_N \end{array}\right\} [s = 1, 2, \ldots, n] \ldots(8) $$

If we square and add equation (8) and use equations (2) and (6) we obtain equation (4). In order to determine the nN μ's and the n^2 l's in terms of the given m's we therefore have the nN equations (8), the $\frac{1}{2}n(n-1)$ equations (7), the n equations (2), and the n equations (6) of which, as we saw, n are not independent. So we have $nN + \frac{1}{2}n(n+1)$ independent relations between the $nN + n^2$ undetermined μ's and l's. If by means of equations (8) the μ's be first determined in terms of the l's we then have $\frac{1}{2}n(n+1)$ equations from which to determine the n^2 l's, which can therefore be determined with $n^2 - \frac{1}{2}n(n+1) = \frac{1}{2}n(n-1)$ degrees of freedom: the number of degrees of freedom with which n mutually perpendicular directions can be fixed in n-dimensional space.

We conclude that any n correlated variables can always be expressed in terms of n independent variables, which may be selected with $\frac{1}{2}n(n-1)$ degrees of freedom.

III.

Now suppose that the correlations between the n correlated variables $q_1, q_2, \ldots, q_n$ satisfy the conditions[2] for a hierarchy: namely that

$$ \frac{r_{as}}{r_{at}} = \frac{r_{bs}}{r_{bt}} \quad [a, b, s, t = 1, 2, \ldots, n] \ \ldots\ldots\ldots\ldots\ldots\ldots(9) $$

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[1] Garnett, *loc. cit.* 96, equation (16).

[2] The following equation (9) that expresses these conditions was given by Mr Cyril Burt in this *Journal*, III. 1909–10, 159; and Professor Spearman and Dr Hart in this *Journal*, v. 65, refer to it as 'Burt's equation.' In two previous papers by the present writer—one in this *Journal* and one in the *Proc. Roy. Soc.*—the conditions in question

where r_{as} denotes the correlation between q_a and q_s, and where a, b, s, t have any four different values from 1 to n inclusive. It has been shown[1] that, when these conditions are satisfied, the n q's may be expressed in terms of $n + 1$ independent x's ($x_1, x_2, \ldots, x_n, x_g$) by means of the equations

$$q_s = r_{sg} \,.\, x_g + \sqrt{(1 - r^2_{sg})} \,.\, x_s \quad [s = 1, 2, \ldots, n] \;\ldots\ldots\ldots(10)$$

where r_{sg} is the coefficient of correlation between q_s and x_g, x_g is a *single general factor* occurring in each of the n equations (10), and x_s is a specific factor occurring in the sth of these equations only. There are no group factors.

In the preceding paragraph and in what follows the term 'single general factor' will be used to denote only that one (x_g) of $n + 1$ independent x's which, when n correlated q's whose co relations satisfy equation (9) are expressed in terms of these x's, enters according to equations (10) into each of the q's, while the remaining n independent factors are specific, each entering into a different q.[2]

If now we would know how many of the conditions for a hierarchy expressed in equation (9) are independent, we observe that these conditions led to equations (10) from which it follow by means of the cosine law of correlation[3] that

$$r_{st} = r_{sg}\, r_{tg} \quad [s, t = 1, 2, \ldots, n]. \;\ldots\ldots\ldots\ldots\ldots\ldots(11)$$

There are $\frac{1}{2}n(n-1)$ of these equations. We may solve n of them for $r_{sg}, r_{tg}, r_{ug}, \ldots$, the coefficients of x_g in equations (11). If then we substitute these solutions in equations (11) we have

$$\tfrac{1}{2}n(n-1) - n = \tfrac{1}{2}n(n-3)$$

remaining independent relations between the correlations of the q's. The independent conditions for a hierarchy are therefore $\frac{1}{2}n(n-3)$ in number[4].

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were described as 'Mr Burt's conditions' in order to distinguish them from certain other conditions that were described as Professor Spearman's 'correlation between columns' conditions, or briefly as 'Professor Spearman's conditions.'

But equation (9) which expresses the conditions for a hierarchy was, as Mr Burt acknowledged, immediately deducible from an equation (equation (*f*), *Ztsch. f. Psychol.* XLIV. 85) previously given by Krüger and Spearman in 1906, and was already foreshadowed by Professor Spearman in the *Amer. Journ. of Psychol.* XV. 1904, 274, 275. In these circumstances we shall not continue to describe by Mr Burt's name the conditions expressed by equation (9): they will instead be referred to below as 'the conditions for a hierarchy.'

[1] Garnett, *loc. cit.* 102, equation (35).

[2] Such a definition of a single general factor has already been given (Garnett, this *Journal*, IX. 347) where also the need for such a definition has been pointed out.

[3] Garnett, *loc. cit.* 96, equation (16).

[4] This result was given without proof (Garnett) in this *Journal*, IX. 1919, 347.

IV.

We have said that whenever the coefficients of correlation of n variables $q_1, q_2, \dots, q_n$ satisfy the conditions for a hierarchy expressed in equation (9), these q's can be expressed by means of equations (10) in terms of $n+1$ independent x's of which one is a single general factor and all the rest are specific factors. We proceed to prove that, as Dr Thomson has found to be the case in his dice-throwing experiments, by making use of a sufficiently large number of independent variables $(x_1, x_2, \dots, x_N)$ in terms of which to express the same n q's whose correlations form a hierarchy, we can so arrange that the number of x's having finite coefficients in the expressions[1]

$$q_s = {}_sl_1 . x_1 + {}_sl_2 . x_2 + \dots + {}_sl_N . x_N \quad [s = 1, 2, \dots, n]$$

for each of the q's and therefore capable of being called general factors (although not of course *single* general factors) shall be very small compared with the number of x's that enter as group factors or specific factors into the expressions for the q's. But we shall go on to show that this result is not inconsistent with the conclusion expressed in the first sentence of this paragraph; for we shall prove that a linear transformation of independent variables will result in the q's being expressed according to equations (10) in terms of one single general factor and n specific factors, without group factors.

Let us then express the q's in terms of a very large number, N, of independent x's by means of the equations

$$q_s = \frac{1}{\sqrt{Nc_s}} \text{(sum of } Nc_s \text{ whole } x\text{'s)} \quad [s = 1, 2, \dots, n] \dots\dots(12)$$

in which c_s is a positive proper fraction that represents the proportion of the whole number N of x's that enters into the expression for q_s. Let us further arrange that the number, Nc_s, of whole x's in the expression given by equation (13) for q_1 is made up of

$$NP \frac{c_1}{1-c_1}$$

specific to q_1.

$$+ NP \frac{c_1}{1-c_1} \left\{ \sum_{s=2}^{s=n} \frac{c_s}{1-c_s} \right\}$$

in which the sth term represents the number of x's common to q_1 and q_s only.

$$+ NP \frac{c_1}{1-c_1} \left\{ \sum_{s,t=2}^{s,t=n} \frac{c_s}{1-c_s} . \frac{c_t}{1-c_t} \right\}$$

in which the typical term represents the number of x's common to q_1, q_s, and q_t only.

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[1] It has been proved that each q will be a linear function of the x's (Garnett, *loc. cit.* 95).

$$+NP\frac{c_1}{1-c_1}\left\{\sum_{s,t,u=2}^{s,t,u=n}\frac{c_s}{1-c_s}\cdot\frac{c_t}{1-c_t}\cdot\frac{c_u}{1-c_u}\right\}$$

in which the typical term represents the number of x's common to q_1, q_s, q_t, and q_u only.

$$+\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+NP\frac{c_1}{1-c_1}\left\{\frac{c_2}{1-c_2}\cdot\frac{c_3}{1-c_3}\cdots\frac{c_n}{1-c_n}\right\}$$

which term represents the number of x's common to all the q's, $q_1, q_2, \ldots, q_n$. ....(13)

where $$P\equiv(1-c_1)(1-c_2)\ldots(1-c_n). \qquad\ldots\ldots\ldots(14)$$

By adding the numbers in the successive lines of this expression (13) for the whole number of x's involved in q_1 it will be seen that they amount to Nc_1, for their sum is

$$NP\frac{c_1}{1-c_1}\left\{1+\sum_2^n\frac{c_s}{1-c_s}+\sum_2^n\frac{c_s c_t}{(1-c_s)(1-c_t)}+\cdots\right.$$
$$\left.+\frac{c_2c_3\ldots c_n}{(1-c_2)(1-c_3)\ldots(1-c_n)}\right\}$$
$$=NP\frac{c_1}{1-c_1}\left(1+\frac{c_2}{1-c_2}\right)\left(1+\frac{c_3}{1-c}\right)\cdots\left(1+\frac{c_n}{1-c_n}\right)=Nc_1.$$

Moreover the number of x's common to q_1 and q_2 is made up of the number common to q_1 and q_2 only together with those which enter into q_1 and q_2 as well as into other q's. The number in question therefore is

$$NP\frac{c_1}{1-c_1}\cdot\frac{c_2}{1-c_2}\left\{1+\sum_3^n\frac{c_s}{1-c_s}+\sum_3^n\frac{c_s c_t}{(1-c_s)(1-c_t)}+\cdots\right.$$
$$\left.+\frac{c_3c_4\ldots c_n}{(1-c_3)(1-c_4)\ldots(1-c_n)}\right\}$$
$$=NP\frac{c_1}{1-c_1}\cdot\frac{c_2}{1-c_2}\left(1+\frac{c_3}{1-c_3}\right)\left(1+\frac{c_4}{1-c_4}\right)\cdots\left(1+\frac{c_n}{1-c_n}\right)$$
$$=Nc_1c_2.$$

It follows from the cosine law[1] that the coefficient of correlation between two q's, say q_s and q_t, given by equations (12) is

$$r_{st}=\frac{1}{\sqrt{Nc_s}}\cdot\frac{1}{\sqrt{Nc_t}}\cdot Nc_sc_t=\sqrt{c_sc_t}. \qquad\ldots\ldots\ldots(15)$$

We therefore have

$$\frac{r_{as}}{r_{at}}=\sqrt{\frac{c_ac_s}{c_ac_t}}=\sqrt{\frac{c_s}{c_t}}=\frac{r_{bs}}{r_{bt}}$$

so that the q's given by equations (12) the right-hand sides of which are in the form (13) have correlations that satisfy the conditions for a hierarchy. And yet inspection of the expression (13) shows that the number of general factors (not *single* general factors) is $Nc_1 c_2 \ldots c_n$ and is therefore smaller than the number of group factors common to any two, three, four or more up to $n-1$ of the q's, and therefore very much smaller than the whole number of these group factors; for the number of group factors common to $q_s, q_t, \ldots, q_w$ is $Nc_s c_t \ldots c_w$, and each c is a proper fraction.

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[1] Garnett, *loc cit.* 96, equation (16).

We may now give rules for writing down the successive expressions for $q_1, q_2, \ldots, q_n$ according to equations (12) supposing that

$$r_{1g} \geqslant r_{2g} \geqslant r_{3g} \geqslant \ldots \geqslant r_{ng},$$

so that
$$c_1 \geqslant c_2 \geqslant c_3 \geqslant \ldots \geqslant c_n.$$

RULE:—*First write down as the expression for* q_1

$$q_1 = \frac{1}{\sqrt{Nc_1}} (x_1 + x_2 + \ldots + x_{Nc_1})$$

involving only the first Nc_1 *x's; then write down a similar expression for* q_2 *involving a proportion* c_2 *of the* Nc_1 *x's in* q_1 *and the same proportion of the* $N(1-c_1)$ *x's not in* q_1*; then write down the corresponding expression for* q_3 *involving a proportion* c_3*, first of the* Nc_1c_2 *x's common to* q_1 *and* q_2*, then of the* $Nc_1(1-c_2)$ *x's in* q_1 *but not in* q_2*, then of the* $Nc_2(1-c_1)$ *x's not in* c_1 *but in* c_2*, and then of the* $N(1-c_1)(1-c_2)$ *remaining x's; and so on.*

It is perhaps worth observing that the whole number of x's entering into the expressions for $q_1, q_2, \ldots, q_n$ is $N(1-P)$, where

$$P = (1-c_1)(1-c_2) \ldots (1-c_n).$$

The x's in expression (13) are distributed among specific factors, 2-group factors, 3-group factors, and so on in the same proportions as among new variables that would be respectively obtained by adding together the sums of $Nc_1, Nc_2, \ldots, Nc_n$ x's selected at random from N given x's a very large number, T, of times, the sums of the x's taken in each trial being added together so as to form the new variables. For example, the number of specific factors in q_1 according to expression (13) is

$$Nc_1 (1-c_2)(1-c_3) \ldots (1-c_n),$$

while the number of x's that will occur in the first of the new variables but not in any other in a large number T of trials is

$$NT c_1 (1-c_2)(1-c_3) \ldots (1-c_n);$$

since
$$c_1 (1-c_2)(1-c_3) \ldots (1-c_n)$$

is the probability that any one x will occur in the first and will not occur in the second or third or ... or nth new variable.

In order that it may be possible exactly to follow the above italicised Rule for writing down expressions for q's having correlations that satisfy the conditions for a hierarchy, it is necessary and sufficient that Nc_1, Nc_1c_2, $Nc_1c_2c_3$, ..., $Nc_1c_2\ldots c_n$ should all be integers. And, if only N be chosen large enough, it will be possible to find integers whose ratios to Nc_1, Nc_1c_2, ..., $Nc_1c_2\ldots c_n$ approach unity within any desired degree of approximation. By choosing N large enough it will therefore be possible to write down, according to our Rule, expressions for the successive q's that will make their correlations approach within any desired limits correlations that satisfy the conditions for a hierarchy.

We have therefore proved our proposition that it is possible, by employing a very large number of x's, to express n q's whose correlations satisfy the conditions for a hierarchy so that the number of x's that enter as general factors (not *single* general factors) will be very small indeed compared with the number of group factors and specific factors; but the number of x's employed may have to be infinite and the x's consequently infinitesimal. We have seen too that the distribution of the x's among the q's defined by our italicised Rule is the same as that which would be obtained by taking the average of a very large number of trial distributions so that the same proportion c_1, c_2, ..., c_n of the whole number of x's always enters into q_1, q_2, ..., q_n respectively.

It follows that, by employing a very large but not an infinite number N of x's for expressing, as nearly as possible according to our italicised Rule and according to equations (12), a much smaller number n of q's, it will be possible to give to the q's correlations which are nearly *but not quite* equal to correlations that satisfy the conditions for a hierarchy. Moreover the number of x's that enter as general factors will, as before, be very small compared with the number of group factors or specific factors; and may indeed be zero.

For example Dr Thomson[1], by choosing $N = 145$ independent x's by means of which to express $n = 10$ correlated q's has, without employing general factors, obtained expressions for the q's which make their correlations approximately satisfy the conditions for a hierarchy. The distribution of Dr Thomson's 145 independent x's is shown in the following table in which the columns given are rearranged from the table published by Dr Thomson[2] so as to show how nearly the x's of successive q's are chosen according to our italicised Rule (which, as we have just seen, would make the correlations of the q's satisfy the conditions for a hierarchy if only a sufficient number of independent factors had been employed). The first 36 columns of the table show the arrangement of group factors, and the last column shows the number of specific factors in each of the ten q's, $q_a, q_b, \ldots, q_l$. The figures above the first 36 columns mark the order of the corresponding columns in Dr Thomson's table. Dr Thomson tells us that the average difference between the correlations of the q's in this table and a set of correlations that satisfy the conditions for a hierarchy is only 0·016. It may, however, be shown that the chance of obtaining so close an approximation to the satisfaction of the conditions for a hierarchy in a random distribution of group factors

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[1] This *Journal*, VIII. 1916, 271 *et seq.*

[2] *Ibid.* 277.

	32	33	34	35	36	1	2	7	10	4	14	11	5	3	8	12	15	13	9	26	28	29	30	23	6	24	17	21	20	25	16	27	31	18	19	22	s
q_a	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	–	–	–	–	–	–	–	0
q_b	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	×	–	–	–	–	–	–	–	–	–	–	–	0
q_c	×	×	×	×	×	×	×	×	×	×	×	×	–	–	–	–	–	–	–	×	×	×	×	–	×	×	–	–	–	×	–	–	–	–	–	–	1
q_d	×	×	×	×	×	×	–	–	–	–	–	–	×	×	×	×	×	–	–	×	×	–	–	–	–	×	–	–	–	×	×	×	–	–	–	–	3
q_e	×	×	×	–	–	–	×	–	–	–	–	–	×	–	–	–	–	–	×	–	–	×	–	–	–	×	×	–	–	–	–	–	×	×	–	–	9
q_f	–	–	–	–	–	–	–	×	×	–	–	–	–	×	–	–	–	–	–	–	–	×	–	–	–	–	×	–	×	–	–	×	–	–	×	×	14
q_g	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	×	–	×	–	–	–	–	–	–	–	×	–	–	–	–	×	–	×	–	16
q_h	–	–	–	–	–	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	×	×	–	×	×	–	–	–	–	–	–	–	–	20
q_k	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	–	–	×	×	–	–	–	×	–	–	–	–	×	×	–	×	–	–	–	–	–	22
q_l	–	–	–	–	–	–	–	–	–	–	–	×	–	–	–	–	×	–	–	–	–	–	–	×	–	–	–	–	×	–	–	–	–	×	–	×	24

is less than one in 10^{11} or less than one in one hundred thousand million. But in view of the fact that, as we saw above (p. 248), the average of a large number of random distributions of the x's in given proportions among the q's is a distribution that satisfies the conditions for a hierarchy, we shall not be surprised if any particular chance distribution of a very large number of x's among a much smaller number of q's yields correlations between the q's that are not very far from satisfying these conditions.

V.

Since the n q's expressed by means of equations (12) according to our italicised Rule in terms of a very large number N of independent x's have correlations that satisfy the conditions for a hierarchy, it must also be possible to express these n q's in terms of $n+1$ independent variables of which one is a single general factor while the remainder are specific factors.

Let us define $n+1$ new variables $g, \xi_1, \xi_2, \ldots, \xi_n$ by means of

$$g = \frac{1}{\sqrt{N}}(x_1 + x_2 + \ldots + x_N) \quad \text{......................(16)}$$

and

$$\xi_s = \sqrt{\frac{1-c_s}{Nc_s}} \cdot (\text{sum of the } Nc_s \ \ x\text{'s in } q_s)$$

$$-\sqrt{\frac{c_s}{N(1-c_s)}} \cdot (\text{sum of the } N(1-c_s) \text{ remaining } x\text{'s}) \, [s = 1, 2, \ldots, n]. \quad (17)$$

It is easy to see that these $n+1$ new variables are independent of each other; for the cosine law[1] gives as the correlation of g and ξ_s

$$r_{g\xi_s} = \frac{1}{N}\left(\sqrt{c_s(1-c_s)} - \sqrt{c_s(1-c_s)}\right) = 0$$

and it is equally easy to verify that the same cosine law gives as the correlation between any two ξ's, say ξ_s and ξ_t,

$$r_{\xi_s\xi_t} = 0 \quad [s, t = 1, 2, \ldots, n].$$

Moreover, employing the expressions for g and ξ_s given by equation (16) and (17), we obtain

$$\begin{aligned}\sqrt{c_s}\,.\,g + \sqrt{1-c_s}\,.\,\xi_s &= \sqrt{\frac{c_s}{N}}\Big\{(\text{sum of all the } x\text{'s})\\ &\quad + \frac{1-c_s}{c_s}(\text{sum of the } Nc_s \quad x\text{'s in } q_s)\\ &\quad - (\text{sum of the } N(1-c_s) \quad x\text{'s not in } q_s)\Big\}\\ &= \frac{1}{\sqrt{Nc_s}}(\text{sum of the } Nc_s \quad x\text{'s in } q_s)\\ &= q_s \qquad [s = 1, 2, \ldots, n]. \quad \ldots\ldots\ldots\ldots(18)\end{aligned}$$

Thus we have proved that the n q's which are expressed by means of equations (12) according to our italicised Rule in terms of a very large number N of independent x's may, by means of the linear transformation of variables given in equations (16) and (17), also be expressed by means of equations (18) in terms of $n+1$ independent variables g, ξ_1, ξ_2, $\ldots$, ξ_n of which one, g, is a single general factor while all the others (ξ's) are specific factors.

Comparing equations (18) with equations (10) and making use of equation (15) we have

$$r^2_{sg} = c_s = \sqrt{\frac{c_s c_t \,.\, c_s c_u}{c_t\, c_u}} = \frac{r_{st} r_{su}}{r_{tu}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(19)$$

giving r_{sg} and c_s in terms of the correlations of the q's.

We have thus verified that while n q's, expressed by means of equations (12) in terms of an infinite number of infinitesimal independent variables (x's) according to the Rule given on p. 248, have correlations that satisfy the conditions for a hierarchy, these same n q's may alternatively be expressed in terms of $n+1$ independent variables of which one is a single general factor while the other n are specific factors. We have moreover given a linear transformation of the independent variables which enables us to replace the large number N of x's employed in the first form of expression by the $n + 1$ independent variables employed in the second form. If we would explain in non-mathematical terms why these two forms of statement are equivalent to one another we may point out that, as equation (16) indicates, the very large number of x's, from among which given numbers occur according to the 'all or none' law of equation (12) in each of n correlated q's, may be regarded as interchangeable constituent elements of the single general factor, g. Thus, in equation (16), g is not altered by any increase of x_s and an equal decrease of x_t: any quantity may be taken from any one x and added to any other one or more x's without altering the general factor. It is true that each of these x's enters into the q's in equation (12) according to the 'all or none' law. But as we have seen the satisfaction of the conditions for a hierarchy within any desired degree of approximation, by means of integral numbers of x's employed according to the Rule on p. 248, requires so large a total number N of x's and proportionately large numbers (Nc_1, Nc_2, ..., Nc_n) of x's in each q (q_1, q_2, ..., q_n) that the significance of this 'all or none' law disappears when those conditions are satisfied; for any given fraction of the whole number of infinitesimal x's may then enter into any one of the q's.

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[1] Garnett, *loc. cit.* 96, equation (16).

VI.

It is important to observe that the single general factor and each of the specific factors among $n + 1$ independent variables employed in equations (10) and (18) for the expression of n given q's, whose correlations satisfy the conditions for a hierarchy, is unique. For suppose that the q's are originally expressed in terms of N independent variables x_1, x_2, ..., x_N where $N \geqslant n$, by means of equations

$$q_s = {}_sm_1 . x_1 + {}_sm_2 . x_2 + \dots + {}_sm_N . x_N \quad [s = 1, 2, \dots, n] \dots(20)$$

where

$$ {}_sm_1^2 + {}_sm_2^2 + \dots + {}_sm^2_N = 1. \quad \dots\dots\dots\dots\dots\dots(21)$$

Since by hypothesis the $\frac{1}{2}n(n-1)$ correlations of the q's satisfy the conditions for a hierarchy we may write

$$q_s = r_{sg} . g + \sqrt{1 - r^2_{sg}} . \xi_s \quad [s = 1, 2, \dots, n] \dots\dots\dots\dots(22)$$

where g, ξ_1, ξ_2, ..., ξ_n are $n + 1$ independent variables and where r_{sg} is the correlation between q_s and g. We note that $r_{sg} = \sqrt{\frac{r_{st} r_{su}}{r_{tu}}}$ and is therefore uniquely determined, apart from sign, in terms of the given correlations of the q's. In order to obtain expressions for the $n+1$ new independent variables in terms of the original x's we may write

$$g = l_1 x_1 + l_2 x_2 + \ldots + l_N x_N \text{ ....(23)}$$

where $$l_1^2 + l_2^2 + \ldots + l^2_N = 1 \text{ ....(24)}$$

and $$\xi_s = {}_s\lambda_1 \cdot x_1 + {}_s\lambda_2 \cdot x_2 + \ldots + {}_s\lambda_N \cdot x_N \quad [s = 1, 2, \ldots, n] \text{ ......(25)}$$

where $${}_s\lambda_1^2 + {}_s\lambda_2^2 + \ldots + {}_s\lambda^2_N = 1 \quad [s = 1, 2, \ldots, n]. \text{ ....(26)}$$

If now we substitute in equations (22) the values of q_s, g and ξ_s given by equations (20), (23) and (25), and if we then equate the coefficients of x_p on both sides of the equations, we have

$${}_sm_p = r_{sg} \,.\, l_p + \sqrt{1 - r^2_{sg}} \,.\, {}_s\lambda_p \quad [s = 1, 2, \ldots, n;\ p = 1, 2, \ldots, N], \text{...(27)}$$

a result that might have been obtained directly from the cosine law by observing that ${}_sm_p$ is the correlation between q_s and x_p. Moreover, since $\xi_1, \xi_2, \ldots, \xi_n$ are independent of g and of each other, we have

$$l_1 \,.\, {}_s\lambda_1 + l_2 \,.\, {}_s\lambda_2 + \ldots + l_N \,.\, {}_s\lambda_N = 0 \quad [s = 1, 2, \ldots, n] \text{......(28)}$$

and $${}_s\lambda_1 \cdot {}_t\lambda_1 + {}_s\lambda_2 \cdot {}_t\lambda_2 + \ldots + {}_s\lambda_N \cdot {}_t\lambda_N = 0 \quad [s, t = 1, 2, \ldots, n] \text{....(29)}$$

Substituting for the λ's from equations (27) in equations (28) we obtain

$$l_1 \,.\, {}_sm_1 + l_2 \,.\, {}_sm_2 + \ldots + l_N \,.\, {}_sm_N = \sqrt{r_{sg}} \quad [s = 1, 2, \ldots, n]. \text{...(30)}$$

Including equation (24) we therefore have $n+1$ equations from which to determine the N l's in terms of the given m's; for equations (29) may be deduced from equations (27) and (30). It follows that all the N l's may be determined in terms of any $n+1$ of them. If therefore $N = n+1$ the l's are uniquely determined in terms of the given m's and so also therefore are the λ's. Employing a geometrical metaphor we may express this result by saying that in a space of $n+1$ dimensions g and the ξ's are uniquely determined.

VII.

Professor Spearman and Dr Hart have shown[1] that the correlations of any set of sufficiently varied mental tests that has yet been carried out approximately satisfy the following 'correlation between columns' conditions: namely that, if the variables $q_1, q_2, \ldots, q_n$ measure the n tests in question; and if r_{st} is the coefficient of correlation between q_s

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[1] This *Journal*, v. 1912–13, 51.

and q_i; and if the $\frac{1}{2}n(n-1)$ r's are arranged in the form of a correlation table, thus

$$\begin{array}{ccccc} - & r_{12} & r_{13} & \cdots & r_{1n} \\ r_{12} & - & r_{23} & \cdots & r_{2n} \\ r_{13} & r_{23} & - & \cdots & r_{3n} \\ \vdots & \vdots & \vdots & \cdots & \vdots \\ r_{1n} & r_{2n} & r_{3n} & \cdots & - \end{array};$$

then the correlation between every pair of columns is ± 1. An examination of the mathematical consequences of the fulfilment of the 'correlation between columns' conditions shows that, whenever these conditions are completely satisfied by a set of n *sufficiently dissimilar* mental tests, the correlations (r's) will form a hierarchy so that the n q's must be expressible by means of equations (10) or (18) in terms of $n+1$ independent variables of which one is a single general factor while the remainder are specific factors[1]. Thus it follows from Professor Spearman's and Dr Hart's results that the correlations of any n sufficiently dissimilar mental tests approximately satisfy the conditions for a hierarchy; so that the measures (q's) of any n such tests can be approximately expressed by means of equations (10) or (18) in terms of $n+1$ independent factors of which one is a single general factor while the remainder are specific factors. We may therefore say that, in any set of sufficiently dissimilar mental tests that has hitherto been carried out, a single general factor together with specific factors are dominant, while group factors, although they may occur, are of minor significance; or, in other words, that the qualities tested in any such set of tests *tend* to be made up of a single general factor and specific factors only.

It is true that n q's whose correlations satisfy the conditions for a hierarchy and which can therefore be expressed by means of equations (18) in terms of $n+1$ independent variables may, after a linear transformation of independent variables given by equations (16) and (17), be expressed in terms of a very large number, N, of independent x's by means of equations (12) and our italicised Rule; and it follows that n q's whose correlations approximately satisfy the conditions for a hierarchy may be approximately expressed by means of equations (12) and our italicised Rule.

We have therefore two alternative ways of expressing the consequences of the fact that the correlations of any n sufficiently dissimilar mental tests approximately form a hierarchy. These alternatives are:

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[1] Garnett, *Proc. Roy. Soc.* xcvi. 105.

(*a*) Each of the qualities tested tends to be made up of a single general factor and specific factors only; and

(*b*) Each of the qualities tested is made up of a very large number of elements which tend to enter into the various tests according to the 'all or none' law of equations (12) and according to the italicised Rule on p. 248.

The alternative mode (*b*) of expressing the approximate satisfaction of the conditions for a hierarchy may be put in other words by saying that the approximate satisfaction of these conditions implies that the qualities tested are made up of a very large number of elements of which some (lower level) elements are entirely specific, while other (higher level) elements come into play in different activities; but (1) so that any two given higher level elements both of which enter into any test *tend* to enter in the same ratio (unity or any other) into that test as into any other test into which they both enter; and (2) so that the number of higher level elements that enter as group factors into any two or more tests *tends* always to be proportional to the product of the whole number of elements in the two or more tests in question.

These two qualifying conditions, numbered 1 and 2 in the preceding sentence, are satisfied in dice-throwing tests such as have been employed by Dr Thomson in the paper[1] to which reference has already been made, and in a further paper[2] of recent date. For in such tests the scores, say x_a and x_b, of any two dice that enter both into the sth test measured by q_s and into the tth test measured by q_t enter into both in the same ratio, namely unity[3]. And we have already seen (on p. 248) that on the average of a very large number T of trials, in each of which the same number (say Nc_s) of dice enters into the sth test, the number of x's that enter into any two or more of the tests $q_s, q_t, \ldots, q_w$ is $Nc_sc_t\ldots c_w$ and is therefore proportional to the product of the numbers ($Nc_s, Nc_t, \ldots, Nc_w$) of elements in the two or more tests in question.

Without the two qualifying conditions enumerated in the last paragraph but one, the tendency of the correlations of any set of sufficiently varied mental tests to satisfy the conditions for a hierarchy would not be expressed by that paragraph. Nor would it be expressed by any such statement as

> "The mind, in carrying out any activity such as a mental test, has two levels at which it can operate. The elements of activity at the lower level are entirely specific; but those at the higher level are such that they may come into play in different activities. Any activity is a sample of these elements."

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[1] This *Journal*, VIII. 1916, 271.

[2] This *Journal*, IX. 1919, 337.

[3] See for example Garnett, *Proc. Roy. Soc.* XCVI. 98, equations (22).

unless the qualifying conditions are introduced. Dr Thomson has accordingly been careful to qualify the conclusion we have just quoted by means of the following final sentence:

> "The elements are assumed to be additive like dice, and each to act on the 'all or none' principle, not being in fact further divisible[1]."

We have quoted Dr Thomson's words in order to point out how vast a difference this final qualifying sentence makes. Without it, or without the equivalent qualifying conditions numbered 1 and 2 above, Dr Thomson's conclusion would be no more than an expression in words of the fact[2] that each of *n* correlated variables can always be expressed as a linear function of a sufficiently large number of independent variables some of which are specific factors while the remainder may enter into any two or more of the expressions for the correlated variables in question. In other words, without the final qualifying sentence or its equivalent, Dr Thomson's statement would not express the fact that the conditions for a hierarchy tend to be satisfied by the correlations of any set of sufficiently varied mental tests.

The fact that the correlations between any set of sufficiently dissimilar mental tests *tend* to satisfy the conditions for a hierarchy therefore justifies speaking, either with Professor Spearman (*a*) of a tendency among the qualities tested to exhibit a single general factor as we have defined it and specific factors only, or (*b*) of a tendency among the qualities tested to be expressible, approximately according to equations (12) and the italicised Rule on p. 248, in terms of a very large number of higher level elements that are in effect, as we have seen, small interchangeable parts of Professor Spearman's general factor. But, while waiting for further experimental evidence, it is surely preferable, because so much more simple, to speak and to think of Professor Spearman's general factor together with specific factors than of the large number of elements of which the general factor and the specific factors may be supposed to be made up according to equations (16) and (17).

If we have been right in suggesting elsewhere[3] that the measure, g, of the single general factor is a measure of the subject's Will or power to concentrate attention or (in other words) to concentrate nervous excitement in a particular system of nervous arcs, the various x's may measure the numbers of a subject's neurones rendered active, according to

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[1] This *Journal*, IX. 1919, 344.

[2] Cf. Garnett, *Proc. Roy. Soc.* XCVI. 95.

[3] This *Journal*, IX. 1919, 350.

Lucas's and Adrian's 'all or none' law, in that subject's exercise of his different qualities. The whole number of a subject's neurones rendered active by an effort of his Will would then be proportional to his g.

VIII.

The results obtained in this paper, which is only concerned with variables that are distributed according to the normal law and measured in such units as will give to each the same standard deviation σ, may be summarised as follows:

(1) Any n correlated variables can always be expressed in terms of n independent variables which may be selected with $\frac{1}{2}n(n-1)$ degrees of freedom.

(2) The number of independent 'conditions for a hierarchy' (the satisfaction of which conditions by the correlations of n variables implies that each of these variables is compounded, according to the vector law in equations (10) or (18), of a single general factor and of one of n specific factors) is $\frac{1}{2}n(n-3)$.

(3) Since the correlations between three correlated variables will therefore always satisfy the conditions for a hierarchy, three correlated variables can always be expressed in terms of four independent variables of which one is a single general factor[1] while each of the others is a specific factor.

(4) The single general factor among $n+1$ independent variables employed for the expression of n variables whose correlations satisfy the conditions for a hierarchy is *unique* and is defined as *the* single general factor of any such n variables. But there can always be found an infinite number of general factors of n correlated variables: general factors which must be distinguished from *the* single general factor of n variables whose correlations satisfy the conditions for a hierarchy.

(5) By means of an infinite number N of independent variables $x_1, x_2, \ldots, x_N$ it is possible to express each of n variables $q_1, q_2, \ldots, q_n$ whose correlations satisfy the conditions for a hierarchy in the form $q_s = \frac{1}{\sqrt{Nc_s}}$ (sum of Nc_s whole x's), where $s = 1, 2, \ldots, n$ and where the x's in each q are selected according to the italicised Rule on p. 248. That Rule involves selecting the x's for the expression of each q so that the number of x's common to any two or more q's, say $q_s, q_t, \ldots, q_w$, shall be $Nc_s c_t \ldots c_w$ and therefore proportional to the product of the whole numbers of x's employed for the expression of each of the q's in

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[1] Comparisons of tests of three mental qualities only will therefore throw no light upon the existence of a single general factor.

question. The distribution of the x's among the q's defined in the above Rule is the same as would be obtained by taking the average of a very large number of trial distributions so that the same proportions $c_1, c_2, \ldots, c_n$ of the whole number of x's always entered into $q_1, q_2, \ldots, q_n$ respectively. Moreover, the distribution of the x's that results from this Rule is such that the number of x's entering as general factors (not single general factors) is $Nc_1c_2\ldots c_n$ and therefore, since each c is a positive proper fraction, very small indeed compared with the number of group factors and specific factors. By making N very large instead of infinite n q's whose correlations satisfy, to any desired degree of approximation, the conditions for a hierarchy can be expressed in the same form by following the same Rule as nearly as possible.

(6) There are two equivalent ways of stating the consequences of the fact that the correlations between any set of sufficiently dissimilar mental tests *tend* to satisfy the conditions for a hierarchy. These two alternatives are:

(*a*) The measures $q_1, q_2, \ldots, q_n$ of any set of sufficiently dissimilar mental qualities *tend* to be made up, according to the formula

$$q_s = r_{sg} \cdot g + \sqrt{1 - r^2_{sg}} \cdot \xi_s$$

or the equivalent vector law $q_s = g \cos \theta_s + \xi_s \sin \theta_s$ where $\theta_s = \cos^{-1} r_{sg}$, of $n+1$ independent variables of which one is a single factor and the remainder are specific factors, where r_{sg} is the correlation between the sth quality q_s and the unique single general factor g; and

(*b*) The measures $q_1, q_2, \ldots, q_n$ of any set of sufficiently dissimilar mental qualities *tend* to be expressible—by following the above Rule but not otherwise—according to the formula

$$q_s = \frac{1}{\sqrt{Nc_s}} \text{ (sum of } Nc_s \text{ whole } x\text{'s)}$$

in terms of an infinite number of infinitesimal independent variables $x_1, x_2, \ldots, x_N$ that may be regarded[1] as the constituent interchangeable elements of the single general factor, where c_s $(= r^2_{sg})$ is the proportion of the whole number of x's that enter into the expression for q_s.

(7) It is submitted that of these two equivalent statements, (*a*) and (*b*), the former is preferable because so much more simple.

(8) A suggestion is made for explaining, in psychological terms, the equivalence of the two alternative forms of statement, (*a*) and (*b*).

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[1] Cf. a joint note on the hierarchy of abilities by Garnett and Thomson, this *Journal*, IX. 1919, 368.

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OBSERVATIONS ON THE DE SANCTIS INTELLIGENCE TESTS[1].

By W. B. DRUMMOND.

(*From the Baldovan Institution for Feeble-minded Children.*)

I. *Introductory.*
II. *Description of the tests*
III. *The material required.*
IV. *The nature and claims of the tests.*
V. *The tests applied to normal children. Conclusions as to the grading of the tests.*
VI. *The tests applied to mentally defective children.*
VII. *Resulting criticisms.*
VIII. *Conclusions.*

I. *Introductory.*

In the year 1905 Binet published his first article "On the Measurement of Intelligence" in the *Année Psychologique*. Immediately afterwards de Sanctis[2] published in the same journal a paper on "The Types and Degrees of Mental Deficiency" which was inspired by the same idea, namely that instead of grading defectives by applying to them such loosely defined terms as idiot, imbecile, feeble-minded and so forth, it would be at once more scientific and more practically useful to arrange a series of tests of gradually increasing difficulty which might be used as a standard whereby to measure the actual amount of intelligence present in any given case. The tentative series of tests published by Binet in 1905 was subsequently elaborated by him, and a scale of graded tests was published in 1908, which was further revised and published in its final form in 1911.

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[1] The Author was assisted by a grant from the Medical Research Committee.

[2] Sante de Sanctis: (*a*) "Tipi e gradi d' insufficienza mentale," *Annali di Nevrol.* Naples, 1906. (*b*) "Types et Degrés d'insuffisance mentale," *L'Année Psychol.* No. 12, 1906, 70–84. (*c*) "Mental Development and Measurement of the Levels of Intelligence," *J. of Educ. Psychol.* Nov. 1911, 498–508.

Binet's idea that the intellectual level of a defective child can be indicated in terms of 'Mental Age,' the said Mental Age being ascertained by the application of a scale of graduated tests obtained by experimental observations upon normal children of various ages, has proved to be a most fruitful one. De Sanctis, as has been stated above, shared with Binet the idea of formulating a scale of tests of gradually increasing difficulty by means of which the mental capacity of a defective person might be gauged. He did not, however, possess Binet's fruitful idea of Mental Age, and he did not grade his tests in accordance with the ages at which they can be successfully passed by normal children. His scale consists simply of a series of six problems or tests of gradually increasing difficulty, and de Sanctis was quite satisfied to indicate any subject's capacity by stating how many of the six tests he could pass successfully.

The primary object of the de Sanctis tests differs from that of the Binet tests. The difference may be expressed very simply. The Binet scale is intended to measure the amount of intelligence; the de Sanctis tests are intended to measure the degree of mental defect. Binet himself states that the chief value of his scale is that it affords a child who is suspected of being mentally defective an opportunity of rehabilitating himself. A child who is very backward at school may be reported by the teacher as mentally defective. Such a child may yet be able to pass the tests suitable for his age. If so, however dull and backward he may be, he is quite intelligent and should be taught in an ordinary school, but by a different teacher. The Binet scale may be used for testing normal children, and is also used for gauging the mental status of the mentally defective. The de Sanctis tests are stated by their author to be specially useful for dividing the mentally defective into three grades—idiots, imbeciles, and the feeble-minded or morons. They are recommended for the examination of children between the ages of seven and sixteen.

The de Sanctis scale is certainly far behind the Binet-Simon scale in practical utility. Nevertheless it has seemed worth while to apply it to a series of normal, and of mentally defective children, of various ages, with the objects (*a*) of ascertaining whether the tests are practical and properly graduated; (*b*) of discovering whether the claim made by de Sanctis that his scale is capable of differentiating between mentally defective and normal persons is justified; (*c*) of standardising the method of using the tests; and (*d*) of grading the de Sanctis tests in terms of Mental Age so that they might be available as substitute tests for some of the problems in the Binet-Simon scale. Every one who has worked with the Binet-Simon scale knows how useful a series of substitute tests would be, for it is by no means uncommon to find a child who cannot be successfully tested either because some other person has tested him recently, or because he has been coached in some of the tests by some interested adult or even by some other child who has undergone examination[1].

II. *Description of the Tests.*

As the de Sanctis tests are not well known[2], no translation of them, so far as the present writer knows, having been published in Great Britain, it may be well, before proceeding further, to describe the tests by means of a somewhat free translation of the directions given by their author. The tests should be applied at a time when the subject is at his best, *i.e.* in good humour, and not fatigued, or disinclined to submit himself to examination.

Test 1. Say to the subject "Give me a ball." (Show the child five glass balls of different colours. The observer notes the time it takes the child to respond, and when the response is obtained, covers the balls with a screen.)

Test 2. Say, "Which is the ball you gave me?" (Exhibit the balls arranged in a row. Time, and cover with a screen as before.)

Test 3. "Do you see this piece of wood (a cube)? Pick out all that are like it in that group." (Five cubes, three cones, and two oblong blocks mixed together are shown. Time and screen as before.)

Test 4. "Here is a card. Point to all the figures on it shaped like this piece of wood" (a cube). (The card, described below, shows a series of black squares, oblongs, and triangles arranged in rows.)

Test 5. "Here are some blocks of wood like the figures you have been pointing out on the card." (Twelve cubes of different sizes are arranged upon the table.) "Look at them carefully and (1) tell me how many there are." (The child is expected to count.) (2) "Now tell me which of them is biggest"; (3) "and which of them is the furthest away from you." (Note the time and the mistakes and screen as before.)

Test 6. This test consists in asking the child the following questions: (*a*) "Are big things heavier or lighter than small things?" (*b*) "How does it happen that a little thing sometimes weighs more than a big thing?" (This question is given if the child has replied correctly to the first.)

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[1] Many additional tests will be found in *The Stanford Revision of the Binet-Simon Scale*, by Lewis M. Terman (Warwick and York, 1917), and *The Measurement of Intelligence*, by Lewis M. Terman (Houghton Mifflin Co. N.D.—? 1916).

[2] O. Decroly and J. Degand, *La mesure de l'intelligence chez les enfants. Seconde contribution critique. La méthode de de Sanctis, L'Année Psychol.* 1907, 230–304.

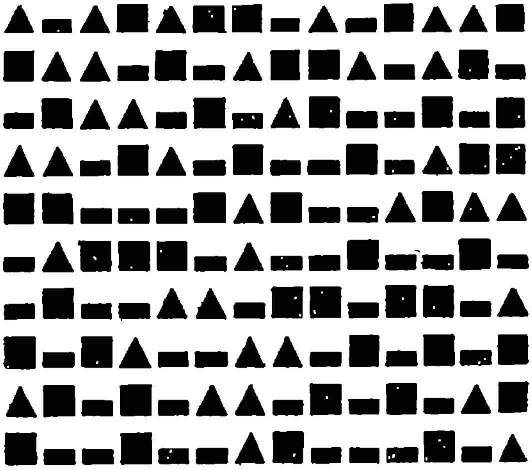
(*c*) "When things are far away do they look larger or smaller than things which are near?" (The object of this question is to prepare the child for the following.) (*d*) "Are they really smaller or do they only appear to be so?" (The object of this question is to ascertain whether the subject is conscious of physiological optical illusions.)

III. *The Material required.*

Test 1. The coloured balls should not be too small. De Sanctis recommends glass balls, but any other material would serve the purpose. A paste-board box will serve the purpose of a screen.

Test 3. The cubes required should measure about 1½ inches. The pyramids should be about two inches high, and have about the same base as the cubes. The oblong blocks should measure 2½ × 1 × ½ inches.

Test 4. The card has ten rows of fourteen figures each. The figures are of three kinds, squares, oblongs, and triangles. The figures have a base of about half an inch. The triangle has the altitude of the square. The oblong is half the height of the square.



The de Sanctis Form-Test. (About one-fourth the actual size.)

Test 5. The twelve cubes are graded in size from half an inch to three inches.

When the tests are given the child must be quite at his ease. Each question may be repeated three times. If the child does not respond, the examination must then be stopped. If there is any doubt as to whether the child has reached his limit, the examination should be repeated again after a few days.

IV. *The Nature and Claims of the Tests.*

In this series of tests the progressive difficulty consists, according to de Sanctis, in the fact that they begin by calling upon the lower mental functions and ultimately make a demand upon the higher. He considers this a more rational procedure than that which consists in giving the subject a series of harder and harder tests of memory or attention. In the mentally defective it is the higher mental functions that are lacking. Many defectives have excellent memories and a very good capacity for attention, which is more than can be said of many quite intelligent people.

De Sanctis claims that by his series of tests one obtains information as to the following: (1) Capacity of adaptation to experience which comprises adaptation to work and certain conditions of attention, of perception, and of will. (2) Immediate memory of colours. (3) The capacity to recognise colours and forms, and to recognise the identity of a plane figure with a solid. (4) The tenacity or duration of attention. (5) The capability of enumerating objects and of judging of their quantity, size, and distance. (6) The capacity to reason about objects no longer present to the senses, and on the general concepts derived from them. This involves not only attention and imagination but also the faculty of generalisation and abstraction. (7) The rapidity of perceiving, of reflection, and of acting.

De Sanctis lays considerable stress upon the rapidity of the child's response, but he admits that this is no gauge of the subject's intellectual level, inasmuch as rapidity of response may depend upon the condition of the sense organs and the muscles, and not solely on the capacity for perceiving, and appreciating.

Bearing de Sanctis's claims in mind, let us now examine the tests seriatim and consider which particular 'functions' are requisite for the solution of the various tests.

Test 1. This test obviously requires an elementary degree of attention, the ability to understand a request, and the capacity to obey a single order, and to carry out a coordinated movement of hand and eye.

Test 2. This test involves in addition the possession of immediate memory for colour.

Test 3. This test involves discrimination of solid form, and self control, if the child stops after picking out all the cubes. Many young children, after picking out all the cubes, thereby showing their capacity to discriminate solid forms, proceed to pick out the pyramids, and the

the oblongs, sometimes arranging these in groups. This unasked for action may result partly from the tendency automatically to continue a pleasurable activity, but may also be in part a manifestation of the acquisitive instinct.

Test 4. To begin with, this test involves the ability to recognise a plane form (the square) as corresponding to a solid form (the cube). It is curious that de Sanctis says nothing about the manner in which the cube is to be held. It is obvious that the cube may be shown to the child in such a way as to present a plane surface of a square shape, in which case it is very easy for the child to recognise the square on the chart as the form demanded; but if the cube were presented with an angle directed to the child and three or four faces visible the problem would be more difficult. In my own observations, the child was shown the cube in such a position that a square surface was plainly visible.

The square being recognised (recognition of plane form), the test then involves the ability to follow the lines of the chart, *i.e.* coordination of hand and eye on a higher level than is required in previous tests, and sustained attention of a very considerable degree.

In examining a number of children, it is quite apparent that the ability to find and follow lines which is acquired during lessons in reading is of great assistance to the subjects. The strain on attention is shown by the fact that mistakes are much more frequently made near the end than in the course of the test. Some children make mistakes at the beginning owing to a failure to understand what they are expected to do, then they perform the greater part of the test without mistake, and finally, becoming fatigued, they make several mistakes towards the end. In many cases mistakes result from hurry or carelessness, whereas more cautious or more deliberate, though not more intelligent, children perform the task perfectly. These variations are of considerable interest, and this test alone throws a good deal of light on the capacity of the subject.

Test 5. This really consists of three different tests, of which the first is much the hardest. This first part (5 *a*)—the enumeration of twelve blocks of different sizes—involves (1) a knowledge of the number series and (2) the possession of the much later acquired fundamental idea in arithmetic of the one to one correspondence.

The second part of the test (5 *b*) requires discrimination and judgment of size, and is the easiest part of the test.

The third part (5 *c*) requires discrimination and judgment of distance. Children often fail in this owing to their tendency to respond as quickly as possible leading them to point to one of the cubes before they have quite grasped what they are expected to do.

Test 6. This really consists of two distinct tests which are not of equal difficulty, though both involve the capacity of generalising from experience. Both tests are open to the objection that they invite guessing, though an experienced examiner will seldom have any difficulty in deciding whether a correct answer is the result of guessing or of understanding. For example if a little girl says that little things may be heavier than big things, and by way of example says that "a weight is heavier than sugar," one sees that the child is speaking from her own experience and one realises the interest with which she has watched the process of weighing sugar at the grocer's. This is the least satisfactory of the de Sanctis tests.

V. *The Tests applied to Normal Children.*

In order to ascertain the value of the de Sanctis tests and attain the aims detailed above, the writer began by testing a series of normal children attending free Kindergartens and Board Schools. The children tested varied in age from three to nine years. Between fifteen and thirty children were tested at each age, with the exception of the first, as only seven three-year old children were available. All the children of three and four, and some of those of five years of age were attending Kindergartens, and came from very poor homes. The three-year old children particularly were very backward physically and mentally, compared to children in better circumstances. The same statement applies to most of the older kindergarten children but not quite to the same extent, as some of them had obviously benefited by their kindergarten training. In the Kindergartens visited all the children were examined. In the Board Schools the teachers were asked to send 'average children' for examination. Whether the children were really average is another question. In carrying on this investigation, and an investigation of the Binet tests, two things were quite apparent and are worth noting. The first was that children of the same age taken from different classes were often at a quite different level of mental development. This will be readily understood, when it is considered that in a large Board School there must be two or three different classes in which the children are of practically the same age. Naturally the more advanced children are put in one class, while the less advanced children of the same age are put in another. Consequently an observer who examined the children of a particular age from one class only might be led astray. The second point was that although the teachers were asked to send average normal children, it was often difficult to get them to attend to that point. Indeed many of them, when they heard that the children were to be examined by a doctor, purposely selected exceptional children in order that they might obtain the observer's opinion upon them. In one school quite a number of exceptional children were noted, and it was found that the infant mistress had been selecting children whom she considered defective in number sense, in ability to learn to read, or in some other way.

Table I.

Table showing percentage of normal children passing the various Tests.

Number tested	7	20	29	23	18	19	16
Age	3	4	5	6	7	8	9
Test 1	100	100	100	100	100	100	100
2	?	95	97	100	94	100	100
3	30	60	100	100	100	100	100
4	—	60	65	91	100	100	100
5 *a*	—	10	38	69	77	100	100
5 *b*	*	55	82	91	94	100	100
5 *c*	—	35	61	86	82	100	100
(1) 6 *a*	—	—	—	—	11	63	56
6 *b*	—	—	—	—	11	36	56
(2) 6 *c*	—	—	40	56	82	84	100
6 *d*	—	—	27	47	72	80	100

* One child correct out of seven—obviously an exceptional case.

Test 1. All the three-year old children tested were successful, and although no children under three years of age were tested, one may safely say that normal children may be expected to be able to pass this test before they are three years of age. Between their second and third birthdays children make a very remarkable advance in their mental development, but individual variations are so great that it is not possible to grade this test more accurately.

Test 2. One minute is supposed to elapse between tests 1 and 2. In some cases the writer applied test 3 in the interval. This, however, is not to be recommended, as it makes test 2 more difficult than if the child's mind had not been so exercised. Nearly all four-year old children succeed with this test, and so do a large proportion of three-year old children. Bearing in mind that the kindergarten children tested were for the most part somewhat retarded in their development, we may fairly place this as a three-year old test.

Test 3. In carrying out this test it is important to avoid giving the child any assistance. The directions should be followed exactly. At most, one may amplify the words used thus—"Pick out all that are like it and put them there," indicating a place on the table. This test, as already mentioned, very often brings out the phenomenon known as automatism. A child lifts the cubes from the group one by one and places them on one side, but instead of stopping he proceeds to remove and set aside the other blocks also. Sometimes he places them among the cubes, sometimes he sets them in another place. These differences should be noted, but if the right blocks are selected first the test should be marked 'passed.' A few three-year old children, a majority of four-year old children, and practically all five-year old children pass this test. A four-year old child who cannot pass may fairly be considered backward.

Test 4. This test may be carried out, as already mentioned, in various ways which present various degrees of difficulty. De Sanctis evidently expects the child tested to follow the card systematically line by line, and to point out every square without mistakes or omissions. Consequently a child should not be counted 'passed' who does not succeed in completing the test in this way without assistance. Complete success involves a good deal more than ability to distinguish the squares from the other figures, for the child is called upon to follow the lines consecutively and to maintain a considerable degree of attention and mental concentration for a sufficient time to complete the task. The test is a valuable one because there are so many degrees of partial success that an observer may very quickly arrange a group of children in classes, thus:

1. Children who fail completely.
2. Children who point to squares and oblongs but omit triangles.
3. Children who point to squares only, but pick them out unsystematically on any part of the card.
4. Children who can point out the squares in a given line, when the line is indicated by the observer.
5. Children who can pick out and follow the lines for themselves but become fatigued and make mistakes towards the completion of their task.
6. Children who are completely successful.

This test may be varied by making the children count the squares. If the squares are counted separately in each line, the process of counting seems to make the test easier, perhaps by making it more interesting. On the other hand it is more difficult for the child to count the total number of squares on the chart, because many children who can recognise the squares easily have no facility in counting beyond ten or twenty.

Many children tested make one or two mistakes at the outset through want of confidence or from not being quite sure of what they are to do, yet the rest of the test is carried out correctly. Such children may be regarded as having been practically successful. In marking the children I made use of the following signs: + = quite correct; +? = one or two mistakes or omissions; − = failed or more than three mistakes or omissions.

It is not till seven years of age that all children are successful or practically successful with this test, but at six years of age twenty-one out of twenty-three were marked + or +?

The test may be graded as a six-year old test. Possibly this may be true only of children who have had some lessons in reading, and consequently know how to follow a line, and how to proceed from one line to another.

Test 5. The twelve blocks must be arranged on the table in such a way that all are easily seen. They should be about an inch apart and the furthest away should not be the biggest. In my own tests I placed the second largest block furthest from the child, while the largest block was placed to the child's left and the third largest to the right. The largest blocks were placed as far from one another in the group as possible in order to make the test of the child's ability to compare sizes more severe. Occasionally, though rarely, a child who is asked to count the blocks will lift them up one by one and set them aside. In such a case the child should not be interfered with until the blocks have been counted, but the blocks must be placed in their original positions before the further questions are asked.

The most difficult part of this test (5 *a*) is the enumeration of the blocks, and it is not till eight years of age that 100 per cent. of the children are successful. Of seven-year old children 77 per cent. pass the first part of the test, 94 per cent. the second, and 82 per cent. the third part. For six-year old children the percentages are 69, 91, and 86.

The test may be graded as a seven-year old test. Although a large proportion of six-year old children are able to pass it, the proportion is less than for test 4.

Test 6. Before the questions for this test are put to the child the blocks used in test 5 should be placed out of sight.

The second portion of the test (6 *c* and *d*, see p. 262) might be expected to be the more difficult, as the interests of young children, especially young town children, are directed to near objects. However, it is really the easier, and calls for little comment, except that it is necessary for the observer to make sure that the child really understands. De Sanctis does not insist that his *ipsissima verba* must be used in putting his tests, but says that words must be used which the child can understand. Hence, as children are concrete thinkers, it seems allowable to express the question thus: "If a man went to the end of the street would he look smaller or bigger?"

Observe that this is the correct order of the words. One should not say 'bigger or smaller,' because many young children have a tendency to repeat the last word heard, and therefore might give the correct answer accidentally if the word 'smaller' were placed at the end of the sentence.

This part of the test is passed by 82 per cent. of seven-year old children, while 72 per cent. answer correctly the further question "Is he really smaller or does he only appear so?" For six-year old children the percentages are 56 and 47.

The first part of this test (6 *a* and *b*, see p. 261) also consists of two questions. The preliminary question is "Are big things heavier or lighter than small things?" It is evident that de Sanctis expects the child to answer that little things may be heavier than big ones, and counts the child wrong who replies "Big things." But is this quite fair? Other things being equal, big things *are* heavier than little ones, and therefore, when a child answers so, it seems only just to interpolate the further question, "Are big things *always* the heavier?" No doubt this is a leading question, but a large proportion of young children answer 'Yes' quite positively. A few who say 'No' may be only guessing, and are to be eliminated by the succeeding question. De Sanctis does not say what answer he expects to the question "How does it happen that a little thing sometimes weighs more than a big thing?" nor how he would mark a child who gives a correct example but no explanation. As one can scarcely expect a child to do more than give a correct example, it seems preferable, and gives more definiteness to the test, to ask for one, thus "Can you tell me any little thing that is heavier than a big thing?" To this question children give such answers as "A weight is heavier than sugar"; "a stone is heavier than a basket"; "iron is heavier than paper."

For the first part of the test (*i.e.* 6 *b*) only 11 per cent. of seven-year old children were successful. Of eight-year old children 36 per cent. passed, and of nine-year old children 56 per cent.

Conclusions as to the Grading of the Tests. The following grading of the de Sanctis tests should be confirmed or modified by the examination of a larger number of normal children[1].

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[1] My paper was written before I saw L. Martin's paper, "A Contribution to the Standardization of the de Sanctis Tests," *Training School Bulletin*, XIII. 1916, 93–110.

Table II.

Test	Age
1	2
2	3
3	4
4	6
5	7
6	9

VI. *The Tests applied to Mentally Defective Children.*

All the defective children examined were resident in Baldovan Institution, near Dundee. The group dealt with consisted of 65 boys—practically all the boys who attend the school attached to the Institution. The girls were left out of account in the meantime, as their examination had not been quite completed. It is believed, however, that the examination of the girls would yield quite similar results. The children were examined individually in a quiet room, and they were, in nearly all cases, quite at their ease during the examination.

The general results obtained are summarised in the following table, which shows the number of children examined, their mental ages as ascertained by the Binet tests, and the number and proportion of the children of each age who succeeded with each of the de Sanctis tests.

Table III.

Table of children tested by the de Sanctis Tests.

(Showing the number of subjects who passed each test *but failed with higher tests.*)

Mental age	No. of children	De Sanctis tests										
		6 (1)	6 (2)	5 *a*	5 *c*	5 *b*	4 *a*	4 *b*	4 *c*	3	2	1
9	3	1	2	—	—	—	—	—	—	—	—	—
8	4	—	2	—	1	1	—	—	—	—	—	—
7	9	—	4	5	—	—	—	—	—	—	—	—
6	9	—	1	2	—	3	—	2	1	—	—	—
5	15	—	—	1	4	3	4	1	1	1	—	—
4	12	—	—	1	2	1	—	2	1	4	—	1
3	9	—	—	—	—	—	—	—	3	3	2	1
2	4	—	—	—	—	—	—	—	—	2	—	2

A comparison of the figures in Tables II and III leads to a very interesting result, for it shows that the grading of the de Sanctis tests for normal children of various ages is correct for mentally defective children *of corresponding mental ages* as ascertained by the Binet-Simon scale. This is shown in the table below.

Table IV.

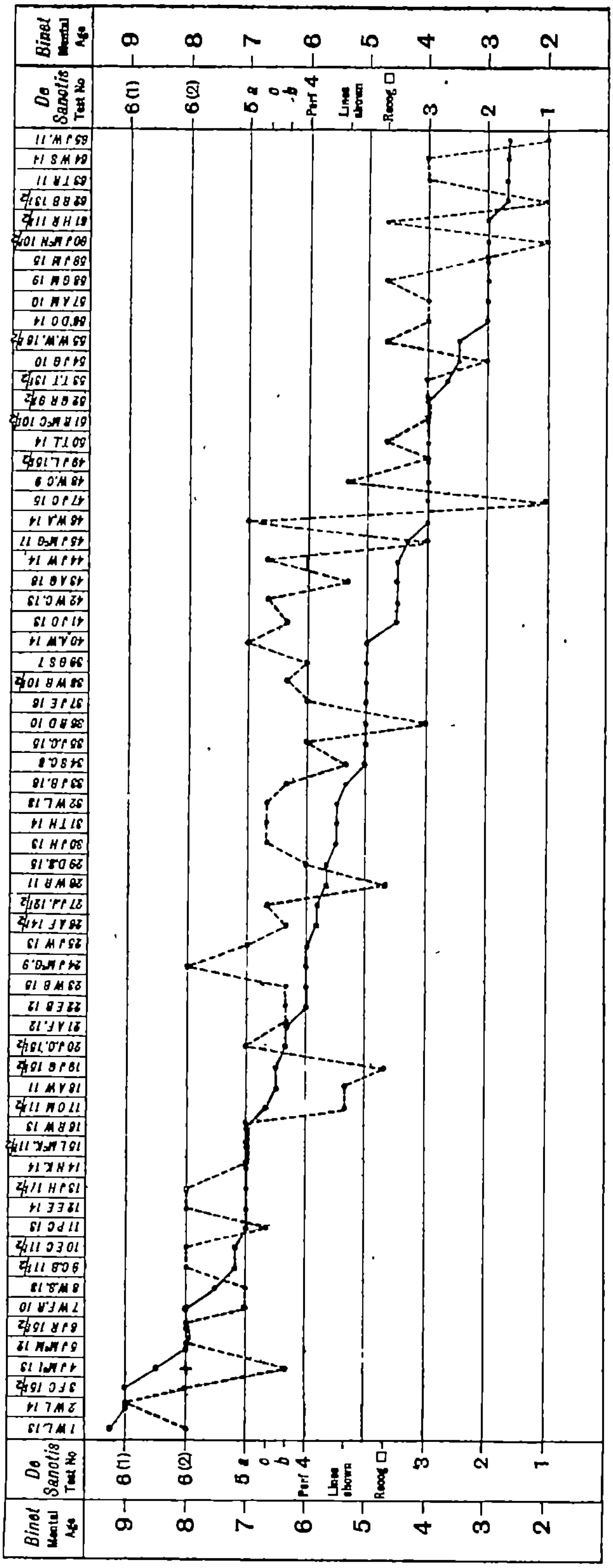
de Sanctis tests	Normal children's age	Defective children of corresponding 'mental ages'
6	9	1 successful out of 3 tested
5	7	10 ,, ,, 13 ,,
4	6	8 ,, ,, 9 ,,
3	4	26 ,, ,, 27 ,,
2	3	8 ,, ,, 9 ,,
1	2	4 ,, ,, 4 ,,

VII. *Resulting Criticisms.*

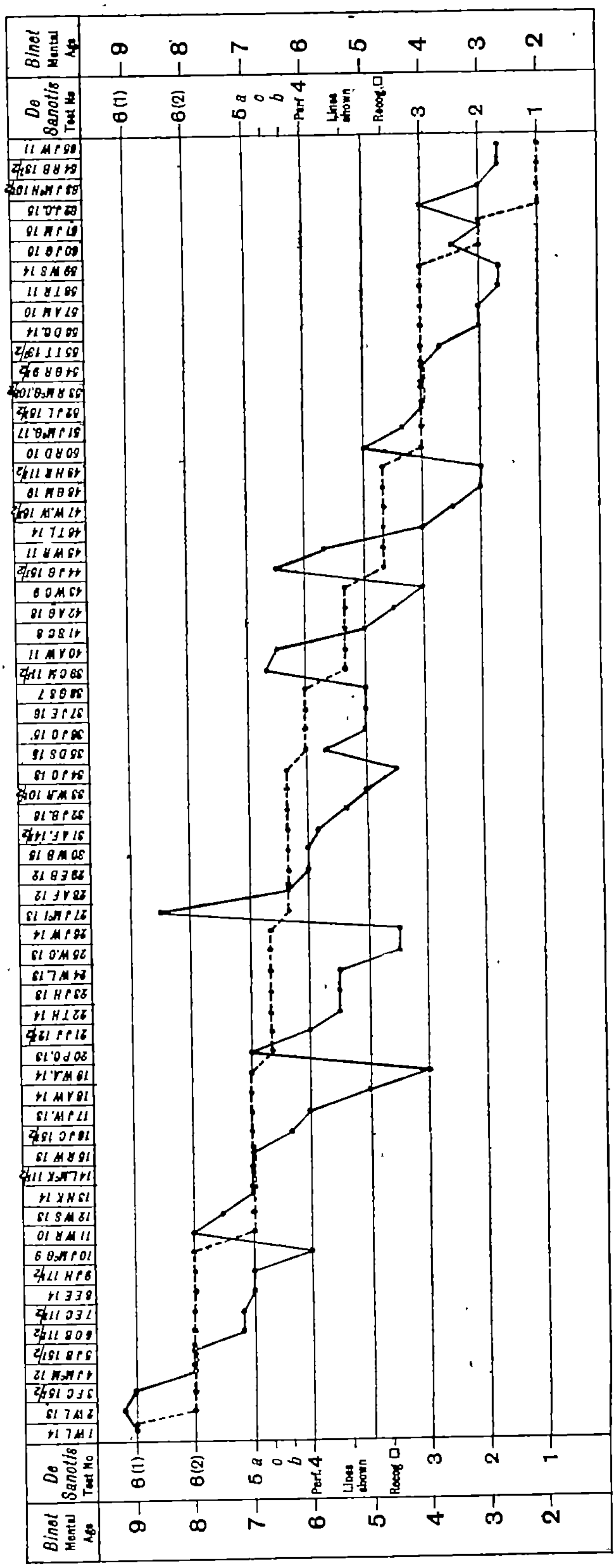
If we now proceed to consider whether these problems really do, as de Sanctis claims, test higher and higher mental functions, it will be found that the contention can hardly be sustained. In the first place several of the tests differ largely in the fact that, like Binet's tests, they make different demands upon the same function. The first four tests, for example, demand progressively a greater and greater degree of attention. In the next place, it is doubtful whether the chief 'functions' tested are really higher in successive problems. The early tests call for the recognition of colour before the recognition of form, but studies of infants and young children show that form is recognised in advance of colour. Again, the tests place the recognition of solid form before the recognition of plane form, which does not appear to be in accordance with the known facts of mental development. It is also a fault that some of the tests contain minor tests of differing degrees of difficulty, whereas each test should be a unit.

Facility in the successful performance of the fourth test is greatly aided by previous instruction in reading; and reading, being dependent upon a special 'centre' in the brain, may be regarded as a special mental 'function.' It does not follow, however, that success with this test proves a child to be in possession of higher mental functions than a child who passes test 3, but does not quite succeed with test 4. The assistance a child gains from lessons in reading is simply facility in finding and following consecutive lines, and this particular facility is, as a matter of fact, acquired by word-blind children.

The first test which really requires the possession of a new 'function' is the fifth, when the subject is called upon to enumerate twelve blocks. It is very probable that number is dependent upon the development of a special brain 'centre.' It is certain that it develops late, and that mentally defective children are frequently, if not usually, more defective in number than in other faculties.


Binet Mental Age
9
8
7
6
5
4
3
2
De Sanctis Test No
6 (1)
6 (2)
5 a
c
b
Perf 4
Lines shown
Recog □
3
2
1

Graph I.


Binet Mental Age
9
8
7
6
5
4
3
2
De Sanctis Test No
6 (1)
6 (2)
5 a
c
b
Perf. 4
Lines shown
Recog. □
3
2
1
1 W L 14
2 W L 13
3 F C 15½
4 J McM 12
5 J B 15½
6 O B 11½
7 E C 11½
8 E E 14
9 J H 11½
10 J McG 9
11 W R 10
12 W S 13
13 N K 14
14 L McK 11½
15 R W 13
16 J C 15½
17 J W. 13
18 A W 14
19 W.A. 14
20 P O. 13
21 J J 13½
22 T H 14
23 J H 13
24 W L 13
25 W.O. 13
26 J W 14
27 J McI 13
28 A F 12
29 E B 12
30 W B 13
31 A F. 14½
32 J B. 13
33 W.A. 10½
34 J O 13
35 D S 13
36 J O 13.
37 J E 13
38 G B 7
39 O W 11½
40 A W 11
41 S C 8
42 A B 13
43 W C 9
44 J B 15½
45 W R 11
46 T L 14
47 W.W 15½
48 B W 13
49 H R 11½
50 R D 10
51 J McG. 17
52 J L 15½
53 R McG. 10½
54 G R 9½
55 T T 13½
56 D O. 14
57 A W 10
58 T R 11
59 W S 14
60 J B 10
61 J W 16
62 J.O. 15
63 J McH 10½
64 R B 13½
65 J W 11

Graph II.

Although the de Sanctis scale does not answer to its author's idea of testing successively higher and higher functions, it is worth while asking whether this idea is likely to be a fruitful one. The present writer would answer this question in the negative. The various mental functions are not built up one upon another in such a way that a higher function does not make its appearance until the lower ones upon which it depends have reached their full, or even a high degree, of development. If it were so, no doubt a series of tests designed to show which functions were present and which were still absent would be of great practical utility, and could be designed without much difficulty.

Although these considerations seem to show that de Sanctis, in choosing and arranging his tests, was to some extent working upon a false ideal, it by no means follows that the tests themselves are without practical utility. To the practical utility of the tests we must now turn our attention.

One noteworthy feature of the de Sanctis tests is that a child rarely succeeds in passing any test higher than a test in which he has failed. In this respect the de Sanctis scale differs from the Binet scale. A child's mental age, according to Binet, is the age all the tests appropriate to which he succeeds in passing, *plus* one year for every five tests successfully passed from higher ages. It is not uncommon to find that a child passes tests several years in advance of some with which he failed. Wallin has shown that epileptics are peculiarly liable to exhibit this phenomenon of 'scattering,' their mental age being frequently made up of 'advance credits' spread over a number of years.

The graphs[1] accompanying this paper show the mental ages of the children according to the Binet scale, but do not show how the children reacted to the individual tests. They show also how uniformly the children attained a definite level on the de Sanctis scale. Where—exceptionally—a child failed in one of the lower tests while passing a higher, the fact is indicated on the graph by the minus sign.

When we examine the graph showing the comparative results obtained by the use of the two scales, it appears at first sight that, although

[1] The graphs show the mental ages of sixty-five defective children as ascertained by the Binet tests, and the de Sanctis tests passed by each child. In Graph I the children are arranged in order according to the Binet tests; in Graph II according to the de Sanctis tests. Binet tests —; de Sanctis tests - - - -. The actual ages of the children are shown beside their initials. The de Sanctis tests are indicated in the margin by number. Recog. □ = child can point out squares accurately but not consecutively; 'lines shown' = child points out all the squares if shown the beginning of each line; 'Perf.' = child passes test without any help. 6 (1) and 6 (2) = parts 1 and 2 of Test 6

there is a very general resemblance between the two curves, the discrepancies are very numerous, and that in many cases the differences are considerable. Closer examination, however, shows that the differences are much exaggerated owing to the fact that there are fewer stations upon the de Sanctis scale. For example between the de Sanctis tests 5 and 6 there is a period of two years. Normal children of seven succeed with test 5, but not until they are nine years old do they succeed with test 6. Accordingly a child whose mental age is eight years according to the Binet-Simon scale might be expected to succeed with de Sanctis test 5 and to fail with test 6. This would lead to his being placed on the seven-year line on the graph (if test 6 were taken as a unit), and his position would appear too low simply from the absence of any station between the seven and nine-year stations on the Binet-Simon scale. To diminish this irregularity I have, for reasons explained above, divided test 6, and children who passed one of the sections of the test are placed upon the eight-year level in the graph.

Where there is a great discrepancy between the results of the two scales, the question arises, which is the more correct? As we possess no independent means of testing the results, I have asked Miss E. Ross, M.A., Headmistress of the School at Baldovan Institution, who very kindly drew the graphs, to express her opinion based upon observation in school. Briefly her opinion may be expressed as follows:

Binet scale: too high—3 cases,
too low— 4 cases,
probably too low— 3 cases.
de Sanctis scale: too high—9 cases,
too low— 5 cases.

In some of these cases the discrepancies can easily be explained by reference to the individual children, in others the explanation is to be found in the nature of the tests. Thus the three children judged too highly placed upon the Binet scale are all comparatively old, their ages being thirteen, fourteen, and sixteen years. These children are making little if any progress in school work, but having much more experience of the world than younger children they are better able to score in a series of tests of general capacity than younger children whose progress in school work makes a better impression on the teacher.

Of the three children considered as placed too low in the Binet scale no general explanation can be given. One of these children suffers from idioglossia and his speech is almost unintelligible. One is partially paralysed.

The most striking discrepancy is found in the large number of cases which appear to be too highly placed in the de Sanctis scale. Some of these are children who have passed a higher test and failed in a lower one, the failure not being allowed for in fixing their position on the graph (failures are indicated by the minus sign); others, on the eight-year old level, were placed there with some hesitation as it was not certain that the child had not succeeded with one of the six tests by confident guessing. When there is any uncertainty as to whether a child is guessing in this test, it is probably wise *not* to give the child the benefit of the doubt.

The de Sanctis tests very rarely seem to give a truer estimate of a child's capacity than the Binet tests. They may occasionally do so when the child is of an essentially practical turn of mind, and especially when speech is defective, as faulty articulation is a cause of failure in several of the Binet tests. Mentally defective children are specially liable to be handicapped by imperfections of speech and by their remarkable backwardness in number. In these two particulars they are, in a large proportion of cases, quite considerably behind normal children approximately on the same level in other respects. On the other hand, a few children seem to be placed too low when tested by the de Sanctis scale. In some cases this may result from poor perception of form—form perception being a necessary part of most of the tests.

On the whole the de Sanctis tests may be regarded as a distinctly useful means of making a preliminary classification of defectives, and they might be used when it is desired to group a number of defective children in as short a time as possible. The fourth test is particularly illuminative, and the manner in which a child tackles this one test and the extent of his success will be found to furnish a very significant indication of the subject's general capacity. The de Sanctis tests cannot, however, be regarded as even an imperfect substitute for the Binet tests. Indeed this could not be expected of such a small number of tests, mostly of a very concrete kind. But the tests may at some future time, in conjunction with other tests yet to be discovered and standardised, be utilised to amplify and extend the Binet-Simon scale. The present Binet-Simon scale is admitted by all experienced in its use to furnish the best preliminary survey of the capacity of defectives, but the future is sure to bring an extended and improved scale which will indicate more clearly the level attained by different functions or capacities.

VIII. *Conclusions.*

(*a*) The claim made by de Sanctis that his problems test successively higher mental functions cannot be sustained.

(*b*) Nor can his claim be sustained that his tests can differentiate between the feeble-minded and the normal. Feeble-minded persons with a mental age of nine years may pass all the tests.

(*c*) The tests are quite practical and afford a rapid means of classifying the mentally defective. They are correctly arranged in order of difficulty.

(*d*) Suggestions for the standardisation of the tests are given in the text.

(*e*) The de Sanctis tests may be utilised as substitutes for some of the tests in the Binet scale, but cannot take the place of that scale.

(*Manuscript received* 8 *October*, 1919.)

PUBLICATIONS RECENTLY RECEIVED.

Psychology from the Standpoint of a Behaviorist. By Prof. J. B. WATSON Philadelphia and London: J. B. Lippincott Co. 1919. Pp. xiv + 429.

The definition of psychology as the science of human conduct, must, if consistently adhered to, result in the identification of psychology with 'behaviourism.' This is the standpoint from which the present work,—the first text book on behaviourism by one of its foremost champions,—has been written. Ridding itself of the mental aspect, the new 'psychology' is able to ignore the relation of mind to body: it is a thorough-going 'psychology without a psyche.' "The reader will find," we are told (viii), "no discussion of consciousness and no reference to such terms as sensation, perception, attention, will, image and the like. These terms are in good repute, but I have found that I can get along without them....I frankly do not know what they mean...." It is interesting to observe the construction, the limitations and the consistency of a psychology skilfully developed on these lines.

Emotion (considered, of course, solely from the aspect of its reactions) is distinguished from instinct by two characteristics: (i) "the shock of an emotional stimulus throws the organism for the *moment at least* into a chaotic state" (196), which does not occur in instinctive actions (and surely also in such emotions as tenderness, sympathy, scorn, subjection, admiration and gratitude!); (ii) "in emotions the radius of action lies within the individual's own organism," and it involves principally the visceral muscular and the glandular tissues, "whereas in instinct the radius of action is enlarged to such an extent that the individual as a whole may make adjustments to the objects in his environment" (197) and the striped musculature of the organism is involved.. Thus the movements of defence and attack are regarded by Dr Watson as instinctive, the accompanying vascular and visceral changes as emotional reactions. But the wide mental differences broadly distinguishing emotions from instincts are ruled out of account. Conation, pleasure and displeasure, coenaesthesis, etc., have no place in behaviouristic psychology; curiosity, *e.g.*, becomes "investigatory behaviour" (399).

Thinking, of course, disappears likewise. "Thought is the action of language mechanisms" (316). It is silent speech, "implicit language," "a constituent part of every adjustment process. It is not different in essence from tennis-playing, swimming, or any other overt activity except that it is hidden from ordinary observation and is more complex and at the same time more abbreviated....To make thought a bodily process like any other act" goes 'against the grain' because "historically 'thought' has always been connected with religion" (325).

Sensations are fully considered, not of course as mental processes (or products) but merely in terms of reaction. Various paraphrases are necessary to avoid slipping back into the older psychology. But now and again we seem to catch the author tripping. Thus "in a weak spectrum the region offering the highest stimulating value to the dark adapted eye is the blue-green" (96). Here he succeeds in avoiding the 'mental' term 'brightness' by substituting the rather doubtful and obscure 'behaviourist' equivalent 'highest stimulating value'; but he surely succumbs in admitting the terms 'dark' and 'blue green'! Indeed is it not obvious that ultimately all behaviour must be described in terms of the senses by which it is observed, although the study of such sensations is barred by the behaviourist from psychology? Visual illusions are mentioned as such, but only in inverted commas; in behaviourist language they must appear as the under- or over-reaction of the eye to acute or obtuse angles, etc.

We may well ask how such a 'psychology,' which is debarred from considering the forms and functions of consciousness, differs from physiology. We are told that whereas the latter deals with the reactions of individual, isolated parts of the organism, the former is concerned with the reactions of the entire organism, which is now "as it were put back together again and tested in relation to its environment as a whole" (20). For our part, we doubt whether the physiologist will be content with this limitation of his work or agree with the behaviourist as to the "entire theoretical independence of the two fields" (21).

As might be expected of the author, the book is generally excellent so far as matter is concerned, and the style is breezily American. It contains some new but not very important observations by the author on infantile emotions, instincts and reflexes, and some first-rate original drawings of the nervous system, larynx and sense organs made from special dissections. There are no references to the works of the many authors whose names occur throughout the text.

An Introduction to Social Psychology. By Dr WILLIAM McDOUGALL. Fourteenth Edition. London: Methuen & Co. 1919. Pp. xxiv + 459. 7*s.* 6*d.*

During the eleven years' existence of this widely read book, it has been enriched by the addition of three supplementary chapters. Of these the first, on 'theories of action,' was written for the fifth edition; the second, on 'the sex instinct,' was written for the eighth edition; and the third, hardly less important than its predecessors, on 'the derived emotions,' now appears for the first time. This chapter and a fresh preface constitute the main changes in the present edition.

The new chapter serves a double purpose. It enables the author to define his position in regard to such emotions as confidence, hope, despondency, despair, regret and remorse, which had found no place in the previous editions of his book, but which have been fully discussed of late by Mr A. F. Shand in his *Foundations of Character.* Dr McDougall also subjects Mr Shand's views of these emotions to a vigorous criticism. Mr Shand calls them 'emotions of desire,' but he also calls them 'secondary emotions.' Dr McDougall invents a new term, 'derived' emotions, at first sight a needless substitution for 'secondary' but warranted in view of a different connotation and of his strong opposition to considering them as 'emotions of desire.'

The cause of this divergence of outlook can be traced, we think, mainly to their fundamentally different conceptions of impulse. According to Dr McDougall (and in accordance with general usage), impulse is essentially a form of conative tendency; according to Mr Shand it is an 'undifferentiated emotion.' Hence for Dr McDougall desire is but thwarted, unsatisfied conative tendency (429), whereas for Mr Shand it arises from the evolution of undifferentiated emotion. So, too, Mr Shand applies the term 'impulse' to self-display and self-abasement, reserving the term 'primary emotion' to pride or vanity and to humiliation or shame which he considers to be respectively evolved from them.

Dr McDougall does not apparently recognise that this is why Mr Shand comes to regard desire as an emotional system while he himself regards it as a conative system. It is not surprising that Mr Shand should term confidence, hope, anxiety, despondency and despair 'emotions of desire,' while Dr McDougall should vigorously oppose such terminology. But again it seems as if Dr McDougall's opposition is not always based on adequate apprehension of his opponent's views. For he describes (428) Mr Shand as regarding "these emotions of desire as *comparable with...the primary emotions...*, *each of these qualities of emotion*...being rooted in or dependent upon the activity of a disposition which has *its own conative tendency and proper end.*" But on reference to Mr Shand's work we find that he definitely states (468) that hope, despondency, anxiety, despair, etc. appear to have no impulses or ends of their own, that they *all have* "*the same end*...the end of the desire to which they belong," and that therefore "they *must be different not only from the primary emotions*, but from all those of which we have hitherto made no mention...all of which have impulses and ends of their own" (the italics are the reviewer's).

Dr McDougall protests (430, 431) against Mr Shand's view that the derived emotions (Mr Shand's emotions of desire) enter into the structure of character. Yet he admits himself (447) that the 'tempers' (hopeful, despondent, anxious, etc.) "are very important as determinants of character"! What he really opposes is the view that they are "forces comparable to the great primary emotional conative tendencies" (431), and he gives reasons for this attitude. He points out that hope is merely a new way in which desire operates when confidence begins to fail, that when hope fails it gives place to anxiety, that when anxiety fails it yields in turn to despondency, and that the latter ends in despair; despair being the turning point where we cease to have a prospective derived emotion, and begin to look back only with the retrospective derived emotion of regret.

Mr Shand includes only the prospective forms among his emotions of desire: Dr McDougall adds the retrospective emotions of regret and remorse; he also recognises sorrow as a special form of regret, namely tender regret, whereas Mr Shand regards sorrow and joy among the primary emotions. We may note, in passing, that in earlier chapters Dr McDougall's former view that sorrow is a primary emotion organized within a sentiment still stands (81, 152); he now holds, apparently, that it is a derived emotion, toned by a primary emotion, and springing from a sentiment.

Dr McDougall makes use of the term 'temper' employed by Mr Shand, but not quite in the same way. He objects to Mr Shand's 'irascible' and 'timorous tempers' on the ground they have reference to the primary emotions and should therefore be called 'dispositions' rather than 'tempers.' On the other hand he extends 'temper' to include all the derived emotions, 'hopeful,' 'anxious,' 'despondent,' etc., tempers, and also 'steadfast,' 'fickle' and other tempers. Both agree in applying the epithets 'violent,' 'gentle,' 'sorrowful' and 'joyous' to temper. Dr McDougall follows Mr Shand in showing how individual variations in temper depend on differences in intensity and in persistence of the conative tendencies. He mentions another important factor,—individual differences in susceptibility to pleasure or pain. On the basis of excess or defect of one or other of these three factors he draws an interesting picture of the way in which the various derived emotions are distinguished from one another.

Problems of Subnormality. By Dr J. E. Wallace Wallin. With an Introduction by Dr J. W. Withers. Yonkers-on-Hudson: World Book Co. 1917. Pp. xv + 485. $3.00.

The writer of this book has spent eight years in the study of abnormal children in institutions for the feeble-minded, epileptic and insane, and in public-school and University clinics. His main thesis is a protest against the rule-of-thumb use of the Binet-Simon tests for the recognition of mental deficiency. In America these tests are being widely applied by "psychological amateurs," by "young teachers just out of the normal school," untrained in research, and unfamiliar "with psychasthenic or psychopathic anomalies." The result is that statements have been there made that from 2 to 3 per cent. of all public-school children, and from 50 to 97 per cent. of persons in reformatories, penal institutions, etc., belong to the class of mentally deficient; whereas the writer would reduce these figures to from less than half to one per cent., and from 10 to 25 per cent., respectively. He maintains that the greater number of cases, graded as mentally deficient, turn out to be merely backward. But when we seek for his distinction between backwardness and mental deficiency (or feeble-mindedness), we are told that "there are types and degrees of backwardness which are quite as irremediable as feeble-mindedness, although the backward children can be brought nearer to the normal standards; the difference is essentially quantitative. There are no pathognomonic signs by which we can infallibly differentiate the one condition from the other." He believes, for instance, that dulness and backwardness, rather than mental deficiency, are responsible for crime, the former leading to active immorality, the latter only to moral weakness.

A series of forcible protests is entered against the generally adopted standard that any one above the age of 15 who fails to pass the XII- and XIII-year Binet and Simon tests is necessarily mentally defective. In the first place, Dr Wallin gives five instances of highly successful farmers and one of a successful housewife, aged between 37 and 59, all of whom "according to the Binet-Simon XIII-year standard of normality...would be feeble-minded." Secondly, he denies that social and industrial competence is dependent solely on intelligence and mental development, arguing that an individual's ability to earn a living (which is the practical test of mental deficiency to be adopted) must also depend on the community in which the individual finds himself, on the degree of permanence of his mental retardation, on the presence or absence of accompanying epilepsy, on the age at which the tests are applied, etc.

For these and other reasons Dr Wallin strongly combats the view that "the Binet scale is wonderfully accurate" and will "tell us to a nicety just where a child stands in his mental capacity," although he admits its "very high value...provided that it is legitimately used (that is, as an aid in mental diagnosis but not as an automatic mental diagnosticon)." He maintains that "it is out of the question to make any sort of satisfactory mental or educational examination of a child in ten or twelve minutes."

Such protests would have carried greater weight if the book had been written with more care, fuller knowledge and less diffuseness. The historical section is obviously incomplete; pages 123–128 contain a mass of ill-digested studies on socially or educationally abnormal individuals; the chapter on Epilepsy lacks the modern knowledge of the subject which a thorough medical training would have given; and there is far too scant recognition of the importance of psychopathic conditions in relation to the "problems of subnormality."

Nevertheless, the broader questions, dealing with the educational, social and preventive aspects of the problem, are ably discussed, and the book cannot fail to rank as a substantial contribution to the literature of the subject.

Lectures on Industrial Psychology. By BERNARD MUSCIO, M.A. Second Edition (revised). London: Routledge & Sons. 1920. Pp. iv + 300. 6*s.* 6*d.* net.

These lectures were first published in 1917 in Sydney, at the University of which they were delivered. Consequently they have not been as well known in this country as they deserve to be. It was the first British book to appear on industrial psychology, and no better work can be consulted by any one seeking a popular, level-headed and fair account of the aims and difficulties of the subject. The chief change in the present edition consists in a fuller and more satisfactory account of Scientific Management.

Human Personality and its Survival of Bodily Death. By FREDERIC W. H. MYERS. Edited and abridged by S. B. and L. H. M. London: Longmans, Green & Co. 1919. Pp. xiii + 307. 6*s.* 6*d.* net.

This well-known work, which is full of material of psychological interest, was originally published in two volumes of about 700 pages each and cost two guineas. In the present edition, the text has been very considerably condensed, and most of the appendices, which occupied about a half of each volume, have been omitted. A portrait and a biographical sketch of the author are included.

Modern Science and Materialism. By HUGH ELLIOT. London: Longmans, Green & Co. 1919. Pp. 211. 7*s.* 6*d.* net.

The six essays in this book are entitled: "The Universe as a Whole," "Matter and Energy," "Life and Consciousness," "The Fallacy of Vitalism," "Materialism" and "Idealism." Their preparation, as the writer tells us in his preface, "has involved a study of many different branches of Science and Philosophy. As I completed my survey of these branches in turn, I usually summed up the conclusions which I had gathered and published them as articles in the reviews...." The following sentences may be quoted to illustrate the author's views. "All that really exists is the material particles of the substance of the nervous system" (196). "Sensations, originally free, begin to hang together in little groups. Here we get matter" (203). "Materialism is the name for the great scheme of associated sensations which represents knowledge" (204). "Sensation is the only fundamental reality attainable" (209). "Mind and matter are equally real, but they are not made of different stuff. Mind is neural activity; matter is associated sensation....That is to say, they are both products and types of *experience*. Hence, too, they are both equally real" (210).

Schriften zur Anpassungstheorie des Empfindungsvorganges. By Prof. JULIUS PIKLER. 1919. Leipzic: J. A. Barth. Part 1. Hypothesenfreie Theorie der Gegenfarben. Pp. viii + 104. M. 8. Part 2. Theorie der Konsonanz und Dissonanz. Pp. 34. M 2.

Professor Pikler's colour theory forms part of a general 'adjustment' theory for all sensations. The different grades of tone-free colours are referred to a mean normal condition. A stimulus is not to be regarded as an excitation in a passive organism, but as inducing a reactive adjustment in the organism. The author propounds his explanation of simultaneous and successive contrast, spatial induction, etc., and enters into lengthy criticism of Hering's theory. The more difficult explanation of the toned colours is based on the black-white series. Blue and yellow correspond to black and white respectively; red and green to mean grey. Ethical and aesthetic analogies are freely indulged in. "Gelb verhalt sich zu Weiss, wie siegreiche Tugend zu kampfloser Unschuld" (!). The theory is highly speculative, and does not seem calculated to initiate research. It is paradoxical to describe it as "independent of hypotheses."

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

General Meeting.

November 15, 1919.	A Study of Nyctopsis, by Ll. Wynn Jones.
	A Psychological Study of God, by Stanton Coit.
January 31, 1920.	Observations on the Suggestibility of School Children, by F. Aveling.
	Some Mental Effects of Alcohol and other Drugs, by May Smith (in collaboration with W. McDougall)

Sectional Meetings.

(a) *Educational Section.*

October 15, 1919	Psycho-analysis, Suggestion and Education, by William Brown.
November 26, 1919.	The Psychology of Child Education, by Maria Montessori (Joint Meeting with Medical Section).
December 18, 1919.	The New Psychology and the New Teaching, by Kenneth Richmond.
January 5, 1920.	The Development of Mental Tests, by P. B. Ballard.

(b) *Industrial Section.*

October 22, 1919.	What Industry expects from Psychology, by P. G. Jackson.
November 28, 1919.	The Present Attitude of the Workers to Industrial Psychology, by S. S. Brierley.
December 19, 1919.	The Causation of Accidents, by E. L. Collis.
January 7, 1920.	A National System of Education, by J. C. M. Garnett.

(c) *Medical Section.*

October 29, 1919.	Suggestion and Suggestibility, by E. Prideaux.
November 26, 1919.	The Psychology of Child Education, by Maria Montessori (Joint Meeting with Educational Section).
December 17, 1919.	Some Physical Signs of Unconscious Wishes and Mental Conflict, by W. H. B. Stoddart.
January 21, 1920.	Recent Advances in Psycho-analysis, by Ernest Jones.

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NOTE ON PROFESSOR J. LAIRD'S TREATMENT OF SENSE PRESENTATIONS

BY J. E. TURNER.

PROFESSOR LAIRD'S article[1] is concerned with the origin of the whole of knowledge, which at every stage however advanced maintains its connexion with our sense experience of the material world. His able treatment appears to furnish further evidence that any theoretical isolation of sense data (as such) from the total apprehended content is much more difficult than is generally admitted. This becomes clear, I think, if we recognise that the original mind content—the "vague voluminous sensory mass" (p. 278)—even though transformed or transcended, always persists as a constituent of experience, and further that the development from this of the higher forms of knowledge is essentially continuous. It is therefore impossible ever to demarcate absolutely, within the total apprehended content, those elements, and those alone, which are contributed by sense, for these have become so interwoven with and penetrated by the products of higher activities that their isolation could be effected only by the abolition of all conscious processes except those initiated by the sense organs alone. This again at once raises the question—What exactly is the 'sense organ'? Plainly it is one thing for physiology, and quite another for psychology; for the latter cannot disregard the connexion between its nerve structures and the cerebral centres; these for psychology all function together, so that it becomes impossible to formulate any perfectly general characters of 'sense content'; as Professor Laird expresses it, perceived content "differs for different subjects according to the character of their several minds" (p. 264); and hence arises the difficulty attending its description to which he alludes on p. 266, and which is really due to the impossibility of expressing in general terms characteristics which are essentially individual and exclusively peculiar.

All that appears feasible therefore is the merely provisional ascription of average characters to a theoretically average experience, leaving room in the case of every individual for very wide limits of departure from this average; in the same way *e.g.* as the definition of a biological species aims at no more than an expression of average characters, from which subspecies, varieties and individuals themselves always differ; and any such interpretation of sense data (meaning by this term "the objects with which we are directly acquainted in sensation and perception" (p. 265)) must be governed by two main principles. In the first place, since developed knowledge always has as its direct object the real world, and since this knowledge has arisen by continuous and insensible gradations from the primal 'sensory mass,' every perceived content therefore must be no other than a portion or aspect of the real world; and secondly, its apprehension must be directly dependent in the main on the activity of sense organs.

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[1] This *Journal*, 1919, IX. 261.

I am not sure that Professor Laird admits this first principle to be more than either a mere possibility, or a matter of indifference to psychology,—"appearances may in all cases be part of the objective world"; they are "facts which the psychologist is bound to take at their face value" (pp. 264, 265)[1]. True; but then what is their "face value"? Exactly as the face value of gold currency cannot be severed by the economist from the underlying world of commerce, so the face value of sense data includes their identity with that part of the real material world which is apprehended by means of the sense organs; it is from this real world that they are always delimited by the psychologist in the first instance in order to constitute the sphere (really more or less arbitrary) of his investigations; and to exclude this fact as to their origin from their face value is to constitute them at once into a fictitious category which it then becomes impossible to describe, classify or relate, simply because all the requisite terms derive their significance from the real world which has however, been made inaccessible by its prior exclusion.

That this is true becomes obvious, I think, when we consider Professor Laird's ascription of meaning to sense data as part of their face value—"meaning belongs to the presentation *in precisely the same sense* as the colour and the shape" (p. 267). The italics here are my own, and are intended to bring out the importance of the second basal principle determining our consideration of sense data. For if we admit that sense presentations as such have meaning, still this cannot be *in the same sense* as shape and colour; for while our consciousness of these latter depends primarily and directly on the sense organs, the consciousness of meaning, even if it can truly be said to depend on sense organs at all, is always indirect and derivative. The sense organs, *i.e.*, can never in themselves alone arouse any consciousness of meaning, which is always the result of mental processes higher than sense, even though they operate on content apprehended through sense[1]. The primary 'sensory mass' from which all experience originates has never, *as sensory*, any true meaning whatever; meaning can arise only through the free play of recollection and imagination becoming co-ordinated into systematic memory and conception. But, though meaning and the purely sensed content differ thus widely in their modes of origin, still they become so indissolubly combined that all perceived content may be said (but from this standpoint only) to have inherent meaning as part of its face value; perception indeed may be regarded as the conferring of meaning upon sensed content; for until meaning arises, nothing is perceived, though somewhat may be sensed; on the other hand if sensed content be absent, we may still retain meaning, but not in perception.

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[1] Cf. also *Problems of the Self*, 28, 29, 55.

But if meaning is a legitimate element in all presentations, then its limits in any given instance are purely arbitrary; we cannot *i.e.* lay down in advance any general "limitation to presented meanings" which, as a distinct class or category, are never transcended. Certainly the variety and vividness of sensed content cause a natural or unconscious tendency to exclude from our analysis of presentations as much content as possible other than the vivid sense content itself; but this procedure has no foundation whatever in principle, and is but the expression of an economy of mental energy—we reduce deliberate thinking as much as possible, and thus respond to sense stimuli as automatically as we can. Such meaning as any presentation exhibits always appears therefore to be as inherent therein as its purely sensible elements; but at the same time this can attain a very wide range, and a modicum of purely sensed content may be submerged as it were in a mass of meaning to such an extent that the usual proportions are quite reversed, and it then becomes a difficult problem to determine how much of the total apprehended content may properly be called a presentation. Thus when the faintest of heart beats indicates life and hope—when a slight precipitate determines innocence or guilt—how much (if we admit meaning at all) is presentation, and how much not? Professor Laird again instances the sparks at a firework display as having an unquestionable presented meaning, in so far as "relation to other parts of the presented field" is apprehended (p. 267). But let us take the case of a signaller receiving a code message by means of faint and distant flashes; how much of the total meaning is here presented? and if not the whole, what general principle governs our limitation? Such a question is obviously fundamental to the theory of the nature of knowledge, and to the rôle therein of perception, conception and thought. It is true that "cognition is the guide of all movement" (p. 275); but our problem is concerned with the analysis of cognition itself, and the determination, so far as is possible in general terms, of the elements contributed to it by the different grades of conscious activity; so that this statement leaves the origin of meaning an open question.

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[1] Cf. *Problems of the Self*, 53 *n.*: "Probably there is no object sensed which is not also judged."

Again, if sense data as such include meaning, it is difficult to understand how they can be other than continuants, at least to some degree[1]. It is true that "whatever is perceived is felt to be fragmentary" (p. 279), but a content though fragmentary may still be continuous or enduring; but if further it contains inherent meaning, it must *necessarily* possess some measure of continuity. For meaning consists essentially in the transcendence of the merely given—of the merely momentary; the mind cannot grasp a meaning which has no persistence or continuance whatever. But in order to constitute a sense presentation, it is obvious that some sensed content must also exist together with the meaning, for otherwise nothing is perceived; and since every presentation (as such) is for the mind an individual whole, it must as a whole participate in the continuance possessed by its inherent meaning. And when Professor Laird comes to consider temporal meaning, he abandons his principle of absolute discontinuance (p. 268); but here, as with meaning, *c'est le premier pas qui coute*; and if presentations be admitted to possess any degree, however slight, of continuance, there is then no difficulty in principle in regarding them as sharing, *mutatis mutandis*, in the real continuity of the material world, of which they themselves are indeed parts.

Such a view appears to simplify very much the whole problem of presentations; for we may supplement the realism which Professor Laird advocates[2] by regarding sense data as always fragments or (perhaps better) aspects of the physical world, determined (for psychology) to be sense data by those conditions under which they are always and necessarily presented to a consciousness debarred by its finitude from an immediate apprehension of the whole of reality. Nevertheless it is always reality which confronts consciousness, from the primitive undifferentiated content we call 'sensory,' to the perceived world of the developed intelligence in which perception, itself always fragmentary, is supplemented by conception and thought. Such a method of treatment plainly permits, and even anticipates, the existence of illusion, hallucination and error, for it regards knowledge as being, certainly in actuality and possibly even in principle, incomplete and defective.

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[1] "Sense presentations are not continuants...our descriptions of presentations are not meant to convey the implication that these are even potentially continuants" (p. 266).

[2] *Mind*, October, 1919.

(*Manuscript received* 4 *January*, 1920.)

REPLY TO MR J. E. TURNER'S NOTE

BY JOHN LAIRD.

MR TURNER seems to be undecided upon the extent of our disagreement and so am I. I certainly did not set out to deny the truth of his conclusion. On the other hand I tried to keep very closely to the standpoint of psychology, and I deliberately refrained from debating the whole problem of sense perception. Mr Turner's argument, as it seems to me, moves too rapidly, and overlooks certain most important questions of psychological fact.

When we speak of the 'physical world' we may mean little or much. Sense data, I suppose, are physical. So are the 'things' of common sense; so are scientific objects like atoms and electrons. It is clear, however, that scientific objects are not perceived at all, and that it is very doubtful whether the 'things' of common sense are perceived directly. The problem of the nature of the physical world, and of the truth of our acquaintance with it, is metaphysical, I think, not psychological. What psychology has to do is to describe precisely what *appears* to A or B or C when they see, or hear, or taste; and this description, I maintain, should not prejudge any metaphysical conclusion whatsoever. We touch, and smell, and look. So do children, and so do other animals. Surely, then, it is essential to find out, with the most scrupulous accuracy, what precisely is presented to normal and abnormal, developed and rudimentary minds. Psychology, indeed, differs from physics in its treatment of these questions precisely on account of its carefulness in this particular; and there is nothing arbitrary in psychological procedure. To describe the differences between sight-space and touch-space; or between the sensitivity of the finger-tips and of the small of the back; or, again, the effect of attention upon sensory appearances, is to institute a scientific enquiry into an important matter of fact.

So I think it is a great mistake not to take presentations seriously enough, and to speak roughly and generally of the physical world when we ought to speak minutely and precisely of the facts directly perceived; and I am afraid I cannot agree with some of Mr Turner's further remarks.

I should like to affirm, unrepentantly and with all the emphasis of Mr Turner's italics, that certain meanings belong to sense presentations *in precisely the same sense* as colour or shape. I am at a loss to understand how Mr Turner (in view of his earlier remarks) can possibly *know* that colour depends directly on the sense-organs and that meaning does not; and I should doubt whether all meaning is acquired in individual experience. The need for brevity, I am afraid, must be my excuse for dogmatism, but I should very much like to know whether Mr Turner would be prepared to sustain his position after reconsidering Stout's argument in his *Manual*, pp. 342 *sqq.*, and especially p. 349. Even, however, if all meaning were acquired, that, on my view, would not affect the issue. The history of my acquirements may be what you will, but I certainly perceive meaningful presentations *now*, and this meaning must be included in any accurate account of the facts of direct perception.

Again, I do not believe that the limits of perceptible meaning are wholly arbitrary. Meaning varies, to be sure. Montezuma perceived comparatively little in the way of meaning when he first saw the horses that the Spaniards brought. Later on, a horse was as meaningful an animal to him as to most of us. And the 'fringe' of meaning is always obscure. On the other hand, there are clear cases of perceived meaning, and equally clear cases of meaning that is not perceived at all. The sphericity of a golf-ball, for example, cannot be seen with the eyes all at once, but it is a perceived visual meaning all the same. For there is 'complication' in this matter (granting the possibility of mistakes). On the other hand, inferences concerning what the golf-ball would do *if* I hit it or *if* Mitchell hit it may be part of the meaning of the golf-ball, but are certainly not perceived meanings.

Mr Turner's further argument that sense data must *necessarily* possess some measure of continuity if they contain inherent meaning seems to me quite confused. There is all the difference in the world between meaning and what is meant. The black marks e-t-e-r-n-i-t-y, for example, signify everlastingness, but the marks themselves are perishing. My view is that our interrupted glimpses of the everlasting hills contain indications of uninterrupted, abiding existence; for these glimpses, I think, are *sign-facts*, and are directly perceived as such. But this abiding existence is certainly not presented to us in sense. It is unusual, indeed, for anything meant (or signified) to be presented as well as the sign or meaning which signifies it. To choose Mr Turner's illustration, a shilling may indicate a whole set of commercial conditions, but, in the general case, the laws of economics are not before my mind when I look at a shilling, although the shilling, for me, is not merely a silvery disc, but has some vague twelve-penny sort of meaning even on the face of it.

And it is irrelevant, I think, to point out, that sense data have a certain evanescent continuance. Anything directly perceived, to be sure, endures as long as the time taken in perceiving it, but my problem concerned the manner of our acquaintance with uninterrupted continuants through interrupted presentations, and I maintained that continuants are not perceived and that what we do perceive are *perishing* presentations which signify continuants. I did not deny that these perishing presentations are really parts of the continuants themselves, and I believe that to be true in fact, although the problem goes beyond psychology. None the less, we are bound to distinguish between our presentations and the continuants. If I look at a candlestick, for example, I call it a steady solid thing on the strength of my perception. My presentations, however, are not steady at all. They fluctuate most appreciably, however earnestly I gaze at the candlestick.

All these points I tried to bring together in my article in this *Journal.*

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A PERFORMANCE TEST UNDER INDUSTRIAL CONDITIONS

By S. WYATT and H. C. WESTON.

(*A Report to the Industrial Fatigue Research Board.*)

1. *Introduction.*
2. *Description of the test.*
3. *Results obtained.*
    (*a*) *Daily and weekly variations.*
    (*b*) *Variations within each test period.*
    (*c*) *Temperature and humidity.*
4. *Conclusions.*
5. *Appendix.*

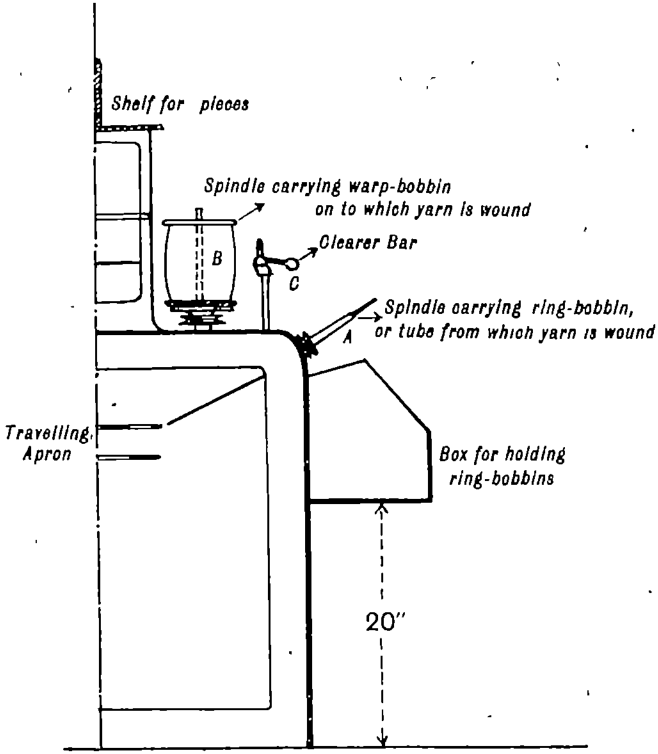
1. Introduction.

The question of a suitable test of fatigue is occupying the minds of many people at the present time. It is generally agreed that the tests already devised and the methods employed are defective in many respects, and cannot be satisfactorily applied to industrial conditions. Those of a psychological nature are usually so different in method and material from the fatiguing industrial processes, that the abrupt change in conditions caused by the application of such a test either during or at the end of a working day may enable the worker to perform the test with renewed interest and energy, and consequently give rise to entirely misleading results. In any case, there must be a considerable number of preliminary trials in order to overcome the effects of unfamiliarity and practice. The existing physiological tests appear to be equally unsuitable and unreliable; those which seem to give the best results being more suitable for the laboratory than for the factory.

2. Description of the Test.

In the course of an investigation into the Cotton Industry carried out under the auspices of the Industrial Fatigue Research Board, it was thought that the essential features of the process under observation might be utilised to form the material for a fatigue test. The process under investigation was that of bobbin-winding in a mill situated in south-east Lancashire. The operation of bobbin-winding, as conducted in this mill, consists in transferring 'ring-yarn' of various lengths from the paper tubes, upon which it has been spun, known as 'ring-bobbins,' on to 'warp-bobbins' large enough to hold an average length of about 20,000 yards. Each operative has charge of one side of a 'winding frame' which contains 100 pairs of spindles carrying the ring-bobbins and the warp-bobbins respectively; and her duties consist chiefly of 'piecing' (*i.e.* connecting up the ends of yarn from the ring to the warp-bobbin) by means of an ingenious instrument (strapped round the palm of the left hand) which ties the ends of the yarn and cuts off the ends close to the knot when the winder presses a small thumb lever.

A section of a winding-frame is shown in the following diagram:


Shelf for pieces
Spindle carrying warp-bobbin on to which yarn is wound
B
Clearer Bar
C
Spindle carrying ring-bobbin, or tube from which yarn is wound
A
Travelling Apron
Box for holding ring-bobbins
20″

The winder gathers the end of the yarn on *A* round the fingers of her right hand, stops the rotation of the bobbin *B* with her left hand, rotates the bobbin in the direction opposite to the rotation of the spindle in order to find the other end of the yarn, and partly lifts the bobbin from the spindle. She then carries the right hand to the bobbin and picks up the yarn end, thus having both ends of the yarn in her right hand. With a circular sweeping movement of the right hand she brings the yarn ends over the knotting instrument, and presses and raises the knotter lever with the left thumb. Lastly she places the yarn in the clearer bar *C* and retards the speed of the bobbin before finally releasing it. These operations constitute the typical movements involved in bobbin-winding, and are repeated several thousand times in the course of a week. It was thought that, if suitably regulated and controlled, they would form the material for a test which would practically represent a cross-section of the actual industrial process and involve the use of those parts of the organism which were normally liable to become fatigued.

A winding frame was accordingly set aside for the purpose of this test, and the partially filled warp-bobbins and the ring-bobbins were placed ready on their respective spindles, with the ends of the yarn from the latter in a visible and uniform position in each case. The winder had then to perform the operations described above. A preliminary test performed by nine winders daily for a week was first given, but it was afterwards decided to apply the test to four winders and to use 50 spindles instead of 100 as originally chosen. The tests were given from 9.30 to 10.0 in the morning and from 3.30 to 4.0 in the afternoon. Thus at the time of the morning test the winders would probably be working at a high level of efficiency. The test was given at 3.30 in the afternoon because tea was served about 4 o'clock and it was thought that the stimulating effect of tea would temporarily neutralise the effect of any fatigue which might exist.

In order to carry out the test, each winder came from her own winding-frame to the test frame and pieced the 50 pairs of ends as quickly as possible. The time required to perform the task was taken by means of a stop-watch recording tenths of a second, and the times taken to knot the first and second twenty-five ends were separately noted in order to obtain an indication of the relative effects of practice and fatigue during the test itself. In addition, the temperature and humidity in the vicinity of the frame at the time of the test were also recorded, together with any special circumstances likely to affect the performance of the winder.

In order to discover the presence of any unusual subjective states which might influence the results of these tests, the winders were supplied with diaries containing questions framed with this object in view. This procedure was found to give very satisfactory results, and there is no reason to doubt the reliability of the replies given to the questions set. Without exception, the winders showed a genuine desire to answer the questions satisfactorily, and the information thus obtained was undoubtedly of considerable value in connexion with the application of the test and the interpretation of the results. These diaries were supplied to the winders weekly and, in order to prevent the possibility of any misapprehension arising as to the object for which the information was required, they contained the following preface.

Please read this before filling up the Diary.

The information you are asked to give in this diary is required for the purpose of enabling the facts disclosed by the test you are performing to be correctly interpreted. There is no desire whatever to pry into your private life unnecessarily, and you are, therefore, not asked to give, for example, any details of how your evenings are spent, but merely to state briefly whether you stayed at home, reading, or otherwise employed, or whether you went for a walk, etc.

You are, of course, under no compulsion to fill up the diary, either wholly, or in part, but as it is hoped that the investigation now being carried out will ultimately benefit you, your assistance and co-operation is invited and will be greatly appreciated.

All the information you give will be treated by the investigator as strictly confidential.

Please write your name on the cover of the diary and hand the latter to the investigator when completed.

The questions contained in the diaries were as follows, a typical set of answers being also given.

Monday, July 14th, 1919.

1.	Time of rising?	6.30 a.m.
2.	Breakfast?	Bread, butter, and cake.
3.	State how you feel this morning.	Well.
4.	Did you take food to the Mill?	No.
	12.15 to 1.15—dinner time.	
5.	Did you have dinner at home or at the Mill?	At the Mill.
6.	State how you feel this afternoon.	Well.
7.	Do you think you pieced the 50 ends as quickly as you did this morning?	Yes.
8.	Did you have tea at the Mill?	Yes.
9.	How did you feel when you left the Mill?	Fairly tired.
10.	How did you spend your evening?	Sewing.
11.	Time of going to bed?	10 p.m.

These questions were suitably modified in the case of Saturdays and Sundays. The importance of these diaries lies, of course, in the fact that any unusual circumstances, such as ill-health or worry, are disclosed, and may account for any unusual changes observed in the reaction-time of the individual.

During the first week of the test the working hours were:

Monday to Friday:	6.0 a.m. to 8.0 a.m.	1st spell
	8.0 ,, ,, 8.30 ,,	Breakfast
	8.30 ,, ,, 12.30 p.m.	2nd spell
	12.30 p.m. ,, 1.30 ,,	Dinner
	1.30 ,, ,, 5.30 ,,	3rd spell

Owing, however, to shortage of work, only 9 hours per day were worked during this week, the operatives going home about 4.30 p.m.

From Monday, June 23rd, till Saturday, June 28th, a strike prevailed in the cotton industry, but on Monday, June 30th, the operatives resumed work according to the following distribution of hours:

Monday to Friday:	7.45 a.m. to 12.15 p.m.	1st spell
	12.15 p.m. to 1.15 ,,	Dinner
	1.15 ,, ,, 5.30 ,,	2nd spell
Saturday:	7.45 a.m. to 12.0 noon.	

Thus, as regards the actual number of hours worked per day, the new conditions were only slightly different from those which prevailed during the first week of the test.

3. RESULTS OBTAINED.

(a) *Daily and weekly variations.*

The times taken by each winder to piece the 50 ends are given on the following page in Table I.

Averages based upon the results of only two winders are enclosed in brackets since they cannot be justifiably compared with the other averages.

The general tendencies of the above results are shown more clearly in Fig. 1.

The most striking feature of the curves of Fig. 1 is the absence of uniformity. The morning and afternoon curves cross each other in the most irregular manner, and in many cases lend no support to the theoretical expectation that the afternoon times would be longer than those of the morning, if fatigue were produced by the winding operations and the test were able to indicate the extent of this fatigue. A certain amount of similarity is noticeable in the morning curves of the individual

Table I.

		Monday		Tuesday		Wednesday		Thursday		Friday		Sat.	Weekly Average		
	Winder	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	a.m.	p.m.	daily
1st week	No. 1	—	—	134·8	141·5	132·8	off	off	off	off	off	off	(133·8)	(141·5)	(137·6)
	„ 2	—	—	161·5	193·2	159·0	160·5	132·0	160·3	138·4	147·1	off	147·7	165·3	156·5
	„ 3	—	—	144·0	157·5	151·0	140·7	147·9	149·1	145·1	155·2	154·0	148·4	150·6	149·5
	„ 4	—	—	139·4	139·6	131·3	145·4	146·2	147·0	141·2	147·1	153·8	142·4	145·0	143·7
	Avg.	—	—	144·9	157·9	143·5	148·9	142·0	152·4	141·6	149·8	(153·9)	144·5	152·7	148·2
2nd week	No. 1	151·9	143·5	130·0	142·1	138·0	138·6	132·4	145·5	148·8	off	141·0	140·3	142·4	141·1
	„ 2	165·9	142·6	150·5	165·3	138·8	154·2	162·9	144·1	169·4	off	144·4	155·3	151·5	153·3
	„ 3	158·1	157·9	162·1	171·6	158·4	168·4	172·5	170·0	150·1	156·5	140·5	156·9	164·9	160·9
	„ 4	140·8	158·4	137·2	154·1	147·8	154·1	142·6	147·5	141·2	162·0	126·1	139·3	155·2	147·2
	Avg.	154·2	150·6	145·0	158·3	145·8	153·8	152·6	151·8	152·4	(159·3)	138·0	148·0	154·2	151·1
3rd week	No. 1	150·0	139·2	132·6	148·0	134·5	130·7	139·0	136·9	144·5	139·1	144·0	140·7	138·8	139·7
	„ 2	148·6	155·0	144·7	137·0	141·3	146·5	157·1	146·5	168·2	149·4	163·3	153·9	146·9	150·4
	„ 3	156·1	150·6	151·9	146·2	156·6	137·7	153·3	163·6	164·5	off	168·1	158·4	149·5	153·9
	„ 4	135·0	133·6	129·7	136·9	146·3	139·9	136·3	143·4	137·2	off	146·4	138·5	138·5	138·5
	Avg.	147·4	144·6	139·7	142·0	144·7	138·7	146·4	147·6	153·6	(144·5)	155·1	147·9	143·3	145·6
4th week	No. 1	153·5	152·5	151·2	131·0	121·9	131·3	126·0	137·8	135·3	129·5	—	137·6	136·4	137·0
	„ 2	159·3	148·3	154·0	144·0	143·1	156·2	144·7	159·5	147·2	150·0	—	149·7	151·6	150·6
	„ 3	170·0	152·0	162·5	147·9	145·0	159·1	145·4	151·7	137·0	143·5	—	152·0	150·8	151·4
	„ 4	148·9	162·4	131·5	139·0	128·1	143·0	140·9	138·6	134·8	129·0	—	136·8	142·4	139·6
	Avg.	157·9	153·8	149·8	140·5	134·5	147·4	139·0	146·9	138·6	138·0	—	144·0	145·3	144·6
Average		153·2	149·7	144·8	149·7	142·1	147·2	145·0	149·6	146·5	(147·9)	(149·0)	146·1	148·9	147·5
Daily Avg.		151·4		147·2		144·6		147·3		(147·2)		(149·0)			

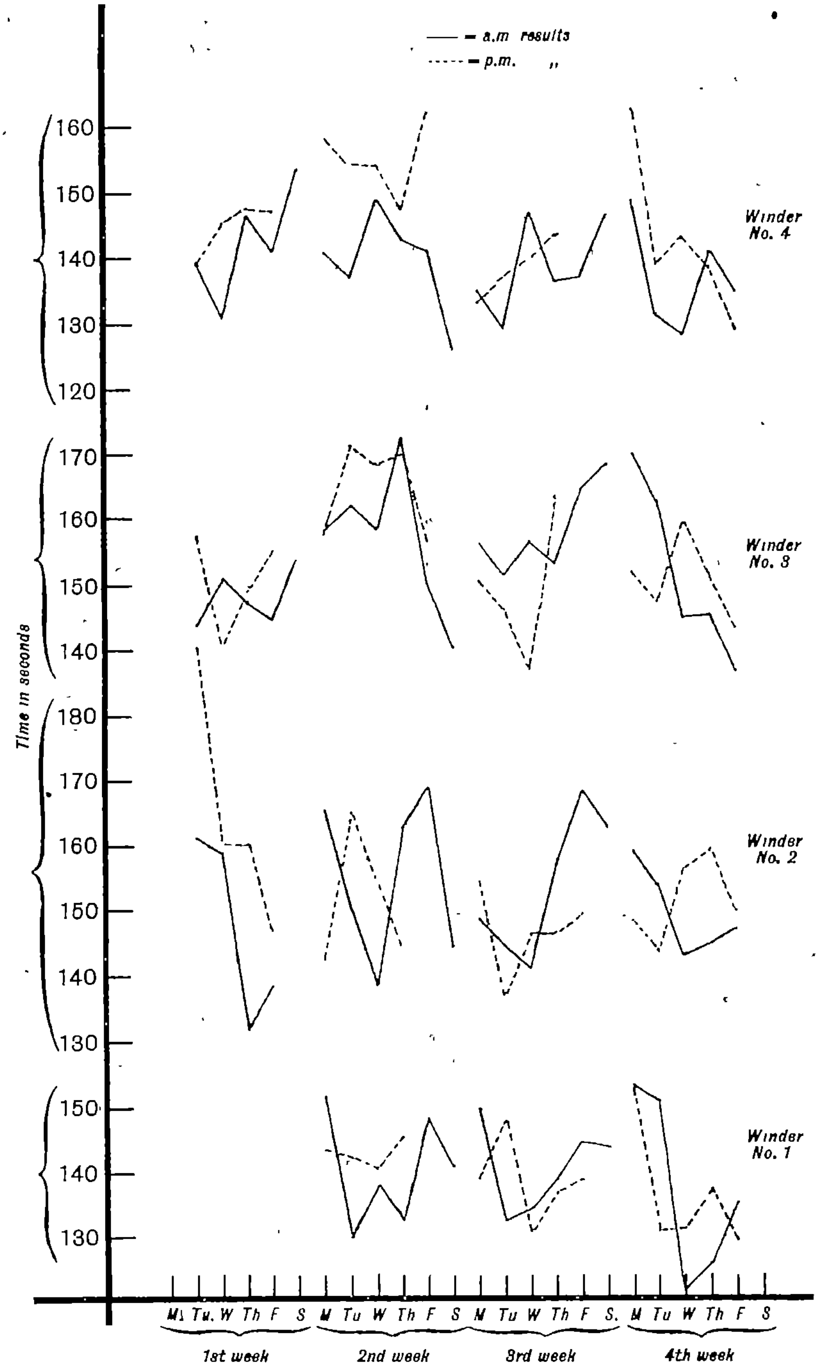

—— = a.m results
------ = p.m. ,,
Time in seconds
160
150
140
130
120
170
160
150
140
180
170
160
150
140
130
150
140
130
Winder No. 4
Winder No. 3
Winder No. 2
Winder No. 1
M Tu. W Th F S M Tu W Th F S M Tu W Th F S. M Tu W Th F S
1st week
2nd week
3rd week
4th week

Fig. 1.

winders, particularly in the case of winder No. 2, but the greatest amount of uniformity in the results of all the winders is to be observed in the fourth week.

The afternoon curves are extraordinarily irregular, and defy all attempts to discover any other common feature. *It would accordingly appear that individual variability increases during the course of the day's work, but becomes less as the subjects become more accustomed to the test conditions.*

The irregularity in the afternoon results may be due to individual differences in susceptibility to fatigue and to incentives which become effective towards the end of the afternoon period, but a more extensive investigation is necessary to determine the existence and nature of such factors. The greater irregularity of the results during the first three weeks of the test is probably due to variation in adaptability to the test conditions.

There is a tendency, especially during the last week of the test, for the Wednesday morning results to be lower than those obtained on the other days of the week. This is probably due to the fact that Wednesday morning marks the close of the winders' week as far as the payment of wages is concerned, and work done after this period is reckoned in the output for the following week. There is consequently a desire on Wednesday morning to work at high pressure in order that the current week's wages may not be adversely affected. It is quite possible that this weekly spurt is unconsciously transferred to the knotting test, or there may be a conscious desire to perform the knotting test as quickly as possible in order to return to the ordinary routine work. It will be seen, however, that the afternoon results on Wednesday of the third week are lower in the case of three winders, than the morning results on the same day. This is probably due to the fact that this Wednesday morning was exceptionally dull and depressing while the afternoon was bright and fine. The afternoon results on the following Wednesday show a marked rise and are higher than the morning results for that day. The only noticeable feature of an unusual nature which could conceivably give rise to such results was the fact that the winders were waiting for yarn for an hour prior to the performance of the test. The waiting period would probably cause a loss of swing and feelings of discontent and dissatisfaction since it represents decreased output and smaller wages.

Occasionally the time taken by a winder to perform the test would be prolonged because of the presence of an unavoidable and inevitable factor. Thus winder No. 2 was delayed 10 seconds on the afternoon of

Thursday in the fourth week because the yarn became entangled in her knotter. Similarly winder No. 4 had some difficulty in finding the end of the yarn on the bobbin during the second half of the test on the morning of the last Monday. Although the apparent duration of the delay was noted and the necessary correction applied to the results, it is quite possible that the entire performance of that particular test would be influenced by such disturbing factors. Thus many of the individual deviations and discrepancies are due to variations in the objective conditions, but a more complete discussion of the factors which may be responsible for the existence of these irregularities will be deferred to a later part of this paper.

The results give some interesting indications of the effects of practice. The weekly average of all the tests during the first week is 148·2 seconds. In the second week this value increases to 151·1 seconds, but in the third and fourth weeks it falls to 145·6 and 144·6 seconds respectively. Thus from the second to the fourth week there is a decrease of 4 % in the time taken to perform the test. The lower value of the results of the first week compared with those of the second is partly due to the omission of Monday from the first week's results. The Monday results are usually higher than those obtained on other days and consequently the weekly average for the first week will be favourably affected by the exclusion of this day. It will also be remembered that the mill was closed for a week between the first and second weeks of the test and when work was resumed the hours of work had been changed. Loss of swing during the holiday together with changed conditions of work would tend to increase the test time during the second week of the test.

An interesting individual example of the effect of practice is to be found in the first week's results of winder No. 2. In this particular case the frame on which the test was performed was a little higher than the one on which this winder usually worked and this small variation from the usual conditions was responsible for the well-marked practice effect during the first week.

In general, the results point to the interesting and significant fact that the least variations from the usual conditions of work will interfere with its normal performance, and that even in the case of a test which so closely approximates to working conditions as the one described in this paper the effects of practice are still evident after the test has been in progress three or four weeks.

As the curves become more uniform and similar during the progress of the test, practice appears to have a levelling effect upon the results.

Table II.

		Monday				Tuesday				Wednesday				Thursday				Friday				Saturday	
		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.		p.m.		a.m.	
	Winder	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25	1st 25	2nd 25
1st week	No. 1	—	—	—	—	69·0	65·8	70·0	71·5	65·0	67·8	off	off	off	off	off	off	off	off	off	off	off	off
	,, 2	—	—	—	—	82·0	79·5	108·0	85·2	78·0	71·0	83·0	77·5	69·0	63·0	83·0	77·3	65·5	72·9	73·0	74·1	off	off
	,, 3	—	—	—	—	74·0	70·0	84·0	73·5	70·0	81·0	67·5	73·2	75·0	72·9	75·5	73·6	75·8	69·3	84·9	70·3	82·5	71·5
	,, 4	—	—	—	—	73·0	66·4	70·0	69·6	70·0	61·3	73·0	72·4	69·2	77·0	70·1	77·8	68·9	72·3	74·5	72·6	73·6	80·2
	Total	—	—	—	—	298·0	281·7	332·0	299·8	285·0	281·1	223·5	223·1	213·2	212·9	228·6	228·7	210·2	214·5	232·4	217·0	156·1	151·7
	Avg.	—	—	—	—	74·5	70·4	83·0	75·0	70·8	70·3	74·5	74·4	71·1	71·0	76·2	76 2	70·1	71·5	77·5	72·3	78·1	75·9
2nd week	No. 1	74·2	77·7	69·0	74·5	69·0	61·0	73·0	69·1	75·9	62·1	70·4	68·2	67·5	64·9	76·1	69·4	65·0	73·8	off	off	69·2	71·8
	,, 2	85·4	80·9	75·4	67·2	73·0	77·5	80·0	85·3	70·1	68·7	79·4	74·8	80·5	82·9	69·6	74·5	81·0	78·4	off	off	75·0	69·4
	,, 3	76·1	72·0	85·7	72·2	87·5	74·6	87·6	74·0	83·4	75·0	86·7	81·7	89·6	82·9	85·0	85·0	89·0	61·0	73·5	88·0	69·7	70·8
	,, 4	69·1	71·7	81·0	77·4	68·9	68·3	72·4	71·7	75·7	72·1	77·1	77·0	70·8	71·8	70·5	77·0	80·0	61·2	81·6	77·4	61·5	64·6
	Total	304·8	302·8	311·1	291·3	298·4	281·4	313·0	300·1	305·1	277·9	313 6	301·7	308·4	302·5	301·2	305·9	315·0	274·4	155·1	160·4	275·4	276·6
	Avg.	76·2	75·6	77·8	72·8	74 6	70·4	78·3	75·0	76·3	69·5	78·4	75·4	77·1	75·6	75·3	76 5	78·8	68·6	77·6	80·2	68·9	69·2
3rd week	No. 1	71·5	78 5	68·5	70·7	60·4	68·6	73·6	74·4	64·6	69·9	65·4	65·3	67·5	71·5	65·0	71·9	73·5	71·0	68·7	70·4	72·0	72·0
	,, 2	78·9	69·7	78·0	77·0	67·0	77·7	72·0	65·0	67·4	73·9	65·2	81·3	86·0	71·1	74·5	72·0	83·5	84·7	68·5	80·9	78·2	83·1
	,, 3	85·5	70·6	73·6	77·0	75·6	76·3	70·0	76·2	77·2	79·4	71·2	66·5	80·4	72·9	79·0	84·6	87·6	76·9	off	off	79·0	89·1
	,, 4	68·4	66·6	64·8	68·8	67·5	62·2	71·8	65·1	68·2	78·1	73·4	66·5	70·5	65·8	73·0	70·4	65·1	72·1	off	off	72·0	74·4
	Total	304·3	285·4	284·9	293·5	270·5	284·8	287·4	280·7	277·4	301·3	275·2	279·6	304·4	281·3	291·5	298·9	309·7	304·7	137·2	151·3	301·2	318·6
	Avg.	76·1	71·4	71·2	73·4	67·6	71·2	71·9	70·2	69·4	75·3	68·8	69·9	76·1	70·3	72·9	74·7	77·4	76·2	68·6	75·7	75·3	79·7
4th week	No. 1	70·2	83·3	81·0	71·5	73·0	78·2	63·2	67·8	61·2	60·7	66·0	65·3	61·0	65·0	68·5	69·3	62·3	73·0	62·9	66·6	—	—
	,, 2	82·0	71·3	66·1	82·2	71·1	82·9	65·2	78·8	68·0	75·1	72·0	84·2	64·0	80·7	76·0	83·5	77·0	70·2	80·0	70·0	—	—
	,, 3	83·2	86·8	74·3	77·7	84·5	78·0	75·0	72·9	65·0	80·0	77·8	81·3	74·5	70·9	79·5	72·2	67·4	69·6	72·8	70·7	—	—
	,, 4	75·9	73·0	73·6	78·8	63·9	67·6	64·5	74·5	63·0	65·1	72·0	71·0	70·5	69·5	70·2	68·4	64·6	70·2	62·0	67·0	—	—
	Total	311·3	314·4	295·0	310·2	292·5	306·7	267·9	294·0	257·2	280·0	287·8	301·8	270·0	286·1	294·2	293·4	271·3	283·0	279·7	274·3	—	—
	Avg.	77·8	78·6	73·8	77·6	73·1	76·7	67·0	73·5	64·3	70·2	72·0	75·5	67·5	71·5	73·6	73·4	67·8	70·8	69·4	68·6	—	—

Unfortunately the test was discontinued at the end of a month, but the results suggest that even at this stage the effects of practice had not entirely disappeared. These facts did not become apparent until several weeks had been spent in the treatment of the results, and it was impossible to re-open the test because of the approach of winter which caused the lighting and heating conditions to be very different from those which prevailed during the test. Since several months must pass before the test can be repeated under similar conditions, it was decided to publish the results in their present incomplete form.

(b) *Variations within each test period.*

Since the times required to knot the first and second 25 ends in each test were noted, it is possible to determine the variations in speed within the test itself. The results dealing with this aspect of the test are given in Table II.

The general trend of these results is seen more clearly in Fig. 2, where they are presented in graphical form.

It will be seen that, with the exception of the third week, the curves representing the times required to knot the first and second 25 ends are approximately parallel, but in the fourth week the position of the curves is reversed. This reversal of position begins at the end of the second week, continues during the following week, and is not complete until the beginning of the fourth week.

The parallelism and reversal of the positions are accentuated when the weekly averages are considered. Such results are represented in Fig. 3.

These variations in the results are probably the resultant of the variable effects of interest, practice, and fatigue. Although the test involved the usual operations connected with winding, and was applied under the familiar industrial conditions, the fact that the winder knew that she was being tested would tend to give rise to a certain amount of nervousness during the first few days, the effects of which would be most marked in the early stages of each test. It is highly probable that the increased time required to knot the first 25 ends during the first and second weeks is partly due to this cause.

The test also involved a rhythm which was slightly different from that which prevailed during the ordinary winding operations. In the test the winder had to knot 50 consecutive ends, and consequently was unable to enjoy the short interval which usually elapses as she passes from one spindle to another often some distance away in the ordinary

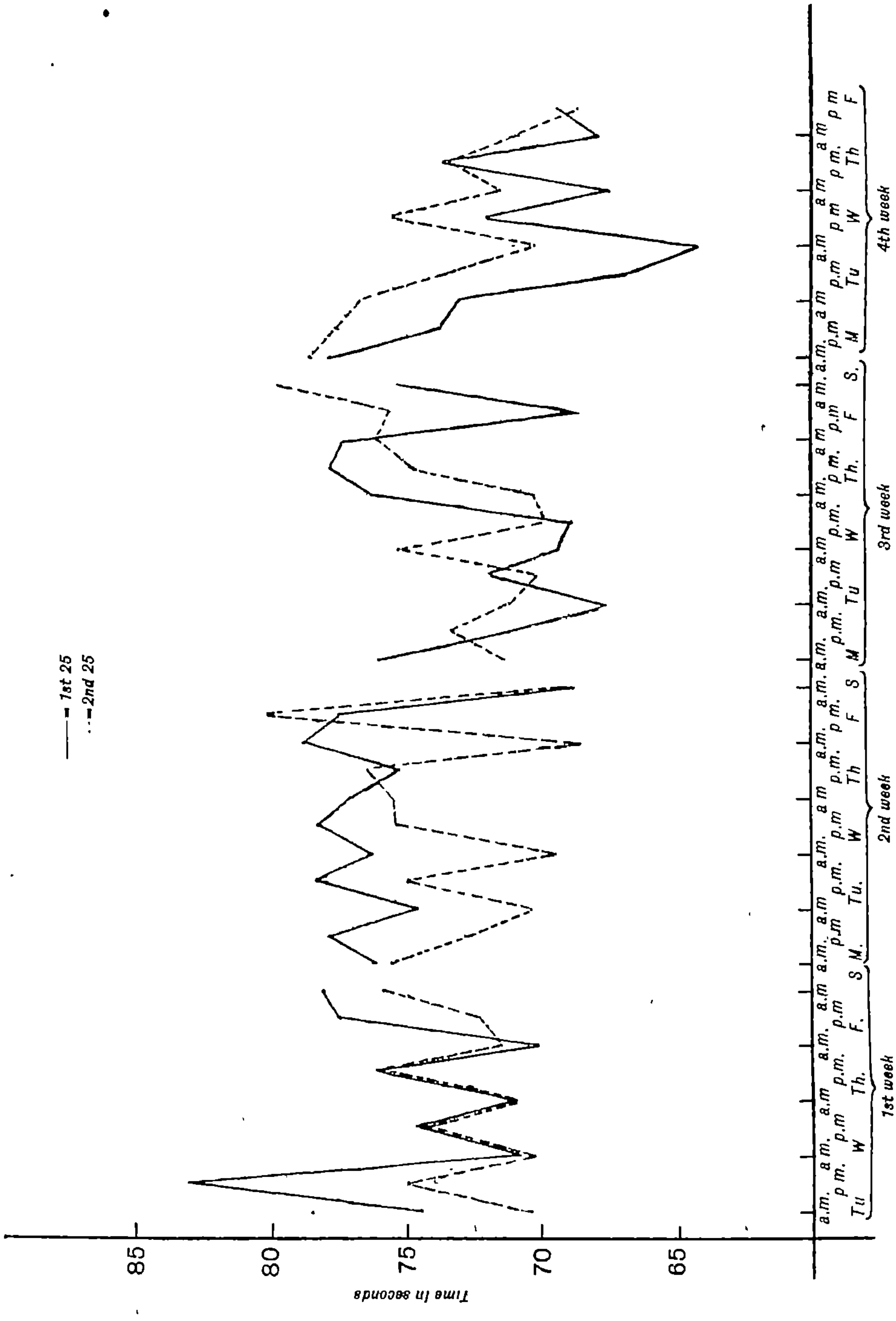

1st 25
2nd 25
Time in seconds
85
80
75
70
65
a.m. p.m. Tu
a.m. p.m. W
a.m. p.m. Th.
a.m. p.m. F.
a.m. S
1st week
a.m. p.m. M.
a.m. p.m. Tu.
a.m. p.m. W
a.m. p.m. Th
a.m. p.m. F
a.m. S
2nd week
a.m. p.m. M
a.m. p.m. Tu
a.m. p.m. W
a.m. p.m. Th.
a.m. p.m. F
a.m. S.
3rd week
a.m. p.m. M
a.m. p.m. Tu
a.m. p.m. W
a.m. p.m. Th
a.m. p.m. F
4th week

Fig. 2.

course of her work. The rhythm required by the test was quicker and more regular than that required by the ordinary winding process, and several trials would be necessary before the winder became thoroughly adapted to the test conditions.

It seems probable, therefore, that the relative position of the two curves during the first two weeks of the test is due to the causes mentioned, and emphasises the importance of the extended application of a test even though it appears to embody only those processes which the subjects have been performing several thousand times daily for many years.

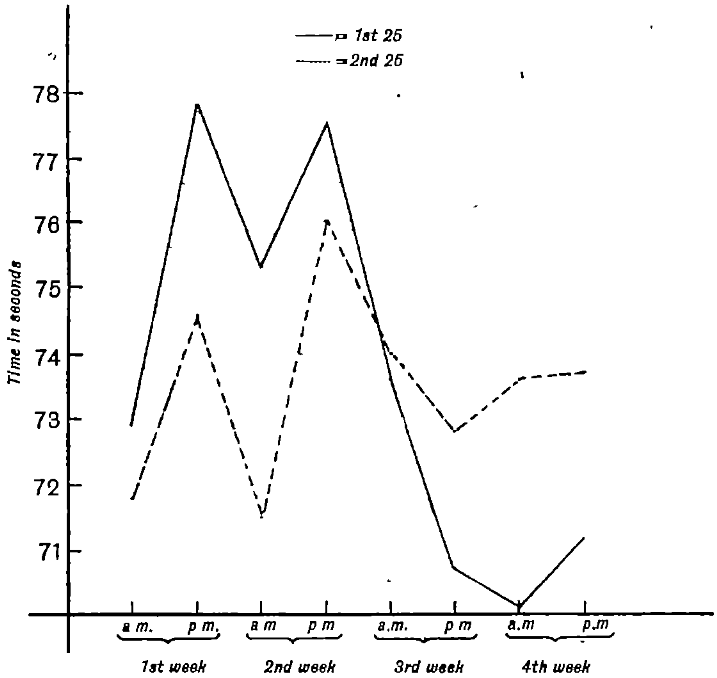


Fig. 3.

The results obtained during the fourth week probably represent the effect of a relatively complete adaptation to the test conditions. The times required to knot the second 25 ends are appreciably longer than the corresponding times for the first 25, a fact which suggests that the test itself gives rise to a certain amount of fatigue. This contention is supported by the remarks made by the winders at the close of the test, to the effect that they felt fatigue of the muscles involved. Thus the nature and duration of the test described is responsible for the existence of a certain amount of local fatigue, a fact which further reduces its value as a suitable test of fatigue.

(c) *Temperature and humidity.*

The temperature in degrees Fahrenheit and the relative humidity of the air in the vicinity of the winding frame at the time of the test are given in the following table:

Table III.

		Monday		Tuesday		Wednesday		Thursday		Friday		Saturday	
Week		a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.	a.m.	p.m.
1st	Temp.	—	—	68	68	66	68	65	65	63	64·5	63	—
	Rel. hum.	—	—	68	68	68	64	68	73	72	67	72	—
2nd	Temp.	63	69	64	62	59	61	60	62	61	62	62	—
	Rel. hum.	63	60	67	72	76	76	71	77	77	72	72	—
3rd	Temp.	60·5	61	61	65	64	66	65	68	66	68	65	—
	Rel. hum.	74	76	76	73	82	78	73	73	68	73	68	—
4th	Temp.	60	63	64	68	66	68·5	67·5	62	68	69	—	—
	Rel. hum.	71	68	63	68	68	73	73	73	73	68	—	—

It will be seen that the amount of variation throughout the test is comparatively small and fails to make any noticeable effects upon the results obtained. The evaluation of correlation coefficients between knotting times on the one hand, and temperature and relative humidity on the other show that no definite correlations exist. Whatever effects the small variations in temperature and humidity may have upon the efficiency of the winder, they are hidden under the influence of other factors.

4. CONCLUSIONS.

Conclusions based upon the results of four operatives over a period of one month can only be of a tentative nature, and may be modified when the investigation includes a larger number of subjects over a longer period. With this reservation, however, it appears that:

1. The time required to knot 50 ends under the conditions described is the resultant of many factors, and the effects of fatigue may be considerably reduced or even entirely hidden by the effects of these other influences. Even when the particular reaction itself has become a long-established habit, as was the case in this investigation, the ever-varying nature of the other psychological, physiological, and physical factors under test conditions tends to modify the results; for fatigue can only be clearly indicated when it has developed to such an extent as to become the dominant factor in the complex situation.

2. Even though the operations involved in the test are similar to those which the winders are in the habit of performing in the course of the ordinary winding operations, about three weeks must elapse before the winders become adapted to the test conditions. The effects of practice are distinctly noticeable throughout the test period, and the least variation from the usual conditions of labour has a disturbing influence upon the results.

3. The results appear to become more uniform and similar as the test progresses, but the afternoon results seem to be more irregular than those of the morning. Thus individual differences decrease as a result of practice, but the day's work has a variable effect upon different individuals.

4. As a means of indicating the amount of fatigue produced in any individual by the industrial conditions under consideration, the test is useless.

5. APPENDIX.

For the benefit and guidance of any investigator who may desire to conduct experiments along similar lines, it may be useful to enumerate and explain the effects of some of the disturbing factors encountered in this investigation. These may be conveniently classified as (i) objective, (ii) subjective.

(i) *Objective.*

(*a*) *Illumination.* The operation of knotting will obviously be affected by differences in illumination since the process involves minute visual discrimination. The mornings of Wednesday, Thursday, and Friday, in the third week of the test, were dull and misty, but the afternoons were bright and fine. It is highly probable that the longer times required to perform the test on the mornings of these days (cf. curve of averages) are due to this fact. Thus variations in illumination at the time of the test may more than neutralise the indications of fatigue.

(*b*) *Humidity.* Since the cooling power of the air depends partly upon the absolute humidity, it is quite possible that differences in the amount of moisture contained by the air at the time of the morning and afternoon tests, or on different days, may affect the results, since bodily comfort and feeling depend partly upon the rate of evaporation of surface moisture.

(*c*) *Temperature.* Efficiency is undoubtedly connected with the temperature of the surrounding air, but the most suitable temperature for winding has never been accurately determined. During the period under consideration moderate temperatures prevailed and the greatest difference between the morning and afternoon temperatures was only 6° F.

(*d*) *Air movement.* Currents of air in the vicinity of the operatives caused either by unequal temperatures in different parts of the room or by machinery movements may exercise a stimulating effect upon the winders, and since these currents are variable the results may be affected accordingly.

(ii) *Subjective.*

(*a*) *Mood.* Variations in the mood of the operative may cause similar variations in efficiency. Remarks made by the winders together with observations of their behaviour disclosed the existence of many different moods which could not be entirely attributed to the presence or absence of varying amounts of fatigue. Sometimes an operator would appear to be depressed and indifferent while at others she seemed bright and happy. The existence of a mood at the time of the test may easily affect the actual capacity for work, and accentuate or diminish indications of fatigue.

(*b*) *Emotions.* Since an emotion is usually more intense and transient than a mood, it is more liable to affect the results of any single test which may be applied at the time of its occurrence. Unless the operatives are able and willing to indicate the nature of their emotional state at the time of the test, it is almost impossible to interpret the results correctly. A winder may be unable to repress the feelings connected with the thoughts of a dance to be held that night; her results will probably indicate the existence of a considerable amount of fatigue, but in reality her reaction times are increased because of the distracting thoughts.

(*c*) *Emotive perseveration tendencies.* These may be variable in their nature and arise from a multitude of causes. A grievance caused by the unjust treatment of a superior, or reflexions upon some domestic trouble may frequently and periodically occupy the focus of consciousness and disturb the operations involved in the test.

(*d*) *Temporary indisposition.* During the course of the investigation it was observed that a winder was slightly indisposed at the time of the morning or afternoon test and was either unable to perform the test or gave such abnormal results that they had to be discarded. Unless the indisposition is noticeable the results may be retained as normal and consequently give rise to incorrect interpretations.

(*e*) *Nervousness.* On one occasion an additional investigator was present during the test, and winder No. 2 was perceptibly nervous. Even when the operatives appear to have become adapted to the presence of the usual investigator, a temporary wave of nervousness may pass over them and affect the results accordingly.

(*f*) *Incentives.* The disturbing effect of periodically or irregularly occurring incentives is very noticeable. The desire to complete a certain number of bobbins within a definite period of time is an instance of such an incentive; and produces a temporary increase in efficiency. The existence of variable stimuli of this nature frequently hides the usual manifestations of fatigue.

Any one or more of these factors may account for the discrepancies observed in the results, but the list enumerated above is by no means exhaustive. It would appear that any test which involves volition and constant attention is liable to be affected by mental and emotional disturbances, and is not likely to be a satisfactory test of fatigue.

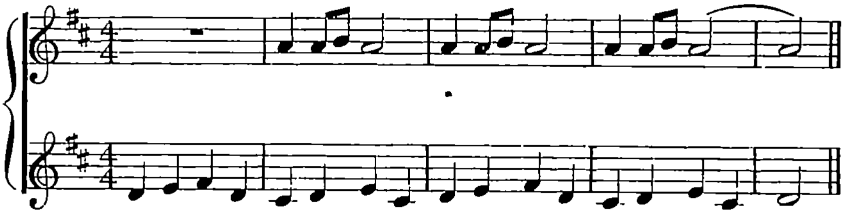
(*Manuscript received* 24 *February*, 1920.)

TWO EXAMPLES OF CHILD-MUSIC

By WILLIAM PLATT.

It is a matter of common knowledge that young children, especially, when in a dreamy mood, will croon or murmur little tunes or scraps of melody which are obviously original and spontaneous. Young children will often sing, instead of say, some of their shorter sentences, making up the tune as they utter it. I have known this happen as early as three years old and as late as twelve. Young children, when two are together, will also sing their little snatches in harmony, instinctively realising what has a good (or passably good) effect. They will also, and this is one of the most remarkable of their achievements, sing in the form of Round or Canon, one of the earliest forms of part-music, inventing as they proceed. Those who wish to follow this subject further will find in my book[1] full details and many examples, including snatches invented by children from 17 months old to four years old, and canons invented by children of three and four years old.

Since that book was published I have heard many other child-tunes, but none of sufficient difference to demand especial mention till this year, when I noted two very fine examples. The first was a bold little piece of part-singing made up by two children (strangers to me) as they walked over Hampstead Heath. The girl looked about 5 or 6 years old, the boy about 7 or 8. Hers was the chief theme, in the lower part; his was the upper counter-subject.



The instinct that guided his counter-subject was an exceedingly good one. And here let me reiterate my contention that the child's spontaneous creation of music is the finest guide to the art-impulse of his nature. He may attempt to draw, but he lacks control of his hand.

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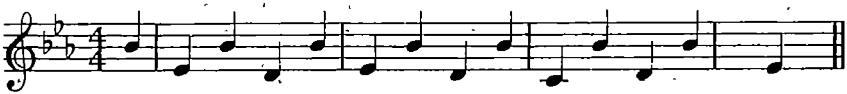
[1] *Child-Music.* London: Renaissance Music Co., Cromwell House, Holborn.

Similarly his aptest self-expression in speech is still inadequate; for this medium also needs much exercise before it becomes fluent enough for real expressive purpose. But in musical expression he is not only far more on an equality with adults, but he is often their superior. How many of the readers of this article would venture to stand up with a friend and improvise a vocal duet? How many, attempting it, would do it as prettily and happily as the example given above? They could only rival the child if they could become as instinctive and as self-unconscious as he. Could they do that, they would probably surprise themselves; for music is an instinct deep down in all of us, and simple spontaneous little musical phrases form the easiest of all art-utterances, whereas the complex orchestral effects form probably the most difficult of all.

The second of the examples to which I have referred above is quite the most remarkable that I have ever noted; it is, in fact, the most interesting specimen of a child-tune ever published. It is a snatch that I heard crooned (with a gentle, humming tone) by a babe only four-and-a-half months old, as he lay contentedly at his mother's breast. It was many times repeated in a dreamy monotonous tone; it was absolutely in tune, despite the somewhat difficult drop of a seventh. The babe who uttered it showed very early the greatest interest in music, and both his parents and all his four grand-parents are musical.



The harmonic basis of this phrase is somewhat similar to that of the duet above noted. Both may be said to be based on the authentic cadence, in contrast to the many examples in which I have shown a distinct tendency in children to favour the plagal cadence; so that it is abundantly evident that children show their temperamental differences in their music.

(*Manuscript received* 28 *January*, 1920.)

A VOICE REACTION KEY.

By ERNEST W. BRAENDLE.

(*From the Cambridge Psychological Laboratory.*)

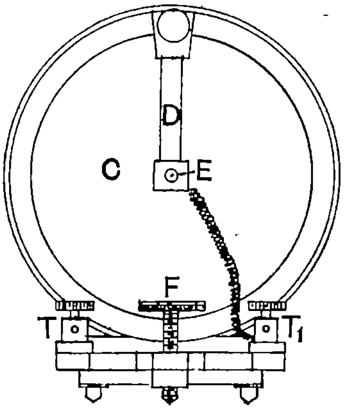
In order to satisfy the requirements of a voice reaction test, it is necessary that either the starting or stopping (or in some cases both) of the Hipp chronoscope be effected by the voice itself. Several devices have been employed for this purpose, but they have generally proved so cumbrous or unreliable, that the voice key has fallen into disrepute.

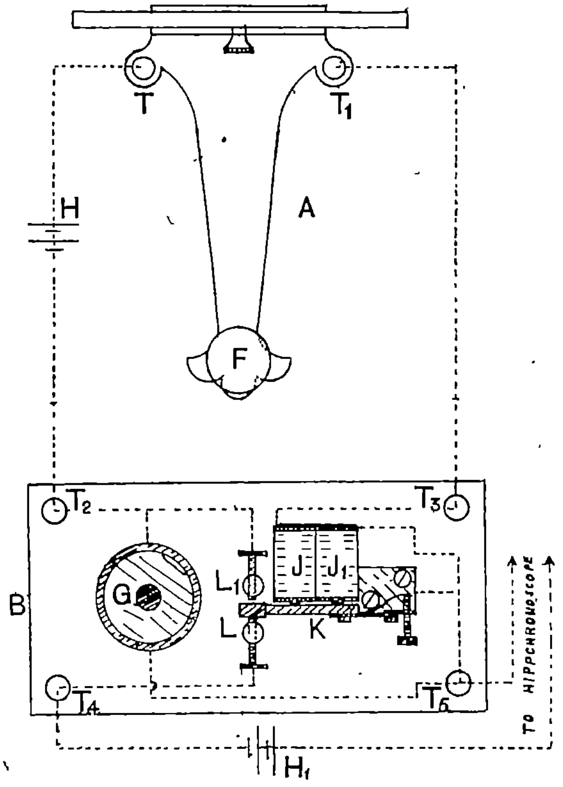
The first model of the apparatus here described was made and tested at the Cambridge Psychological Laboratory, in July, 1919, but has since been improved on, in regard to certain minor details. The apparatus may be said to consist of two parts—(1) the diaphragm circuit breaker *A*, and (2) the relay *B*. By suitably mounting the diaphragm I have been able to actuate the relay with a voice of normal loudness up to a distance of three feet.

(1) The diaphragm circuit breaker *A* consists of a thin metal diaphragm *C*, in the centre of which, and insulated from it, is a small platinum stud, connected by a piece of flexible wire to the terminal T_1, which is insulated from the base. Suspended from the top of the framework by means of a very thin flat steel strip *D* is another platinum stud *E*, which makes contact with the stud in the centre of the diaphragm. A thumb-screw *F* is used for tilting the diaphragm backwards or forwards, in order to allow the stud *E* to swing, so that when at rest it is just touching the stud in the centre of the diaphragm. A terminal *T* is fixed to, and connected with, the framework.

(2) The relay *B* is of the usual bell type, the button-switch *G* being used solely for the purpose of resetting the relay.

Connecting the apparatus up as shown in the sketch, it may be seen that, by pressing the button-switch *G*, the current from the battery *H* is allowed to flow through the electro-magnets *J* and J_1, thus drawing the armature *K* away from the contact-pillar *L*, where it rests normally, over to the contact-pillar L_1, thereby breaking the circuit of the battery H_1 through the Hipp chronoscope and at the same time short-circuiting the button-switch *G*, which may then be released. If, now, one of the


D
C
E
A
F
T
T_1


T
T_1
H
A
F
T_2
T_3
B
G
L_1
L
J
J_1
K
T_4
T_5
H_1
TO HIPPCHRONOSCOPE

various types of apparatus for exposing a syllable be placed in parallel with the terminals T_4 and T_5 and a 'break-make' arrangement be employed, the circuit of battery H_1 will be closed and the Hipp chronoscope hands will remain stationary. Exposure of the syllable will cause the circuit of H_1 to be broken and the hands of the Hipp chronoscope to rotate. If now on the exposure of the syllable a word is spoken on to the diaphragm *C*, the vibration of the diaphragm thus produced will cause the platinum stud *E* to lose contact with the stud in the centre of the diaphragm, thereby for an instant breaking the circuit of battery *H* and allowing the armature *K* to fly back, away from the contact-pillar L_1, thus permanently breaking the circuit of *H* and preventing the armature *K* from returning. The armature *K* is now consequently in contact with the contact-pillar *L* and, the circuit of battery H_1 being 'made,' the hands of the Hipp chronoscope will cease to rotate, thus the time which has elapsed between the exposure of the syllable and the action of the voice on the diaphragm *C* may be noted.

A second diaphragm circuit breaker and a relay, having the contact-pillar *L* on the other side of the armature *K*, which is now extended in that direction, may be used in place of the syllable exposer to start the Hipp chronoscope.

I have also designed an instrument for experimenters who are unable to employ a Hipp chronoscope, the latter being replaced by a stop-watch, measuring to hundredths of seconds. I have entrusted the manufacture of these instruments to Messrs Hawksley of Oxford Street, London, from whom further particulars may be obtained.

(*Manuscript received* 5 *March*, 1920.)

THE DISTRIBUTION AND RELIABILITY OF PSYCHOLOGICAL AND EDUCATIONAL MEASUREMENTS

BY WILLIAM McCLELLAND.

A DIFFICULTY which arises in connexion with the statistical treatment of psychological and educational measurements is that we have, as a rule, a double case of sampling. Not only do we take merely a sample of the individuals, but we take merely a sample of the performances of each. The same problem, of course, arises in Physics and Biometrics, but in the former science the one, and in the latter science the other, source of error is negligible compared with its companion error. In Psychology, however, both are important, and it is the object of the present paper to draw attention to some consequences which have a bearing on the arrangement of a set of measurements and on the treatment of the results obtained.

Let the individuals and the measurements on each individual be normally distributed, with standard deviations σ and σ_m respectively, and in the first instance let us assume that σ_m is the same for all the individuals.

If n be the number of individuals measured, and m the number of measurements made on each; and if Σ be the *apparent* standard deviation of the individuals, each individual being represented by the mean of his m measurements, then it can be shown that

$$\Sigma^2 = \sigma^2 + \sigma_m{}^2/m \quad \text{..............................(1).}$$

It follows that the probable error of the mean value found by making m measurements on each of n individuals is

$$\frac{\cdot 6745}{\sqrt{n}}\sqrt{(\sigma^2 + \sigma_m{}^2/m)} \quad \text{..........................(2).}$$

By giving m and n various values we can find the particular formulae for special cases, as for example making one measurement on each of

n individuals and so on. The total number of measurements is mn, and if this is fixed (say by the time at our disposal) then it is seen that mathematically greater accuracy is attained by increasing the number of subjects as much as possible, making only one experiment on each, if our object is to obtain a value for the racial or class mean[1].

If σ and σ_m can be obtained approximately from experiments already made, formula (1) further enables us to decide the extent to which it is necessary, in the interests of accuracy, to multiply the measurements on each subject. Such increase in the number of measurements does not affect the first term on the right-hand side of (1) but only the second, viz. σ_m^2/m, and if we wish merely to reduce Σ, there is clearly no point in reducing this second term when it is already small compared with the first.

In practice σ is unknown and has to be deduced from Σ and σ_m by writing equation (1) in the form

$$\sigma^2 = \Sigma^2 - \sigma_m^2/m \qquad \text{(3)}.$$

This formula, of course, should not be used to find σ if n or m is small.

Actual experiments with continuous subtraction, where the score was reckoned by the number of pairs of digits subtracted in 10 minutes, gave $\Sigma = 51{\cdot}4$ and $\sigma_m = 11{\cdot}9$.

Hence $\sigma = 50{\cdot}0$.

This shows that with continuous subtraction, neglect of the fallibility of the measurements on the individuals causes an error of about 3 % in the estimate of the standard deviation of the 'true' values for the individuals[2]. With operations where the results are less consistent, and consequently σ_m is much higher, the error caused would naturally be greater, and the question arises whether this factor should not be taken into account in such investigations as that recently carried out by Burt in London[3].

Knowing σ and σ_m we can calculate by formula (2) the probable errors for different numbers of subjects and tests, and we get the following table which shows the relative advantages of increasing the number of subjects and the number of measurements on each subject.

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[1] A different lower limit to the number of measurements on each individual may, of course, be rendered necessary by some special end in view or by psychological considerations.

[2] Of course, as is done above, we should use the measured value of Σ without reduction in finding the probable error of our estimate of the racial mean.

[3] Burt, *The Distribution and Relations of Educational Abilities.*

Table I.

Probable Errors.

Number of subjects	Number of tests					
	1	2	3	4	5	∞
10	10·9	10·8	10·7	10·7	10·7	10·7
15	8·9	8·8	8·8	8·8	8·8	8·8
20	7·7	7·6	7·6	7·6	7·6	7·5
30	6·3	6·2	6·2	6·2	6·2	6·1
40	5·5	5·4	5·4	5·4	5·4	5·3
50	4·9	4·8	4·8	4·8	4·8	4·8
100	3·5	3·4	3·4	3·4	3·4	3·4
1000	1·1	1·1	1·1	1·1	1·1	1·1

It thus appears that with an operation like continuous subtraction there is little use in making more than one measurement on each individual, unless we have some other end in view than finding the position of the mean. With the present results, for instance, the addition of one subject is of as much value as making twenty additional tests on each of the original subjects. In this connection it is interesting to compare Spearman's method of dealing with correlation[1] where he holds that by making more careful experiments on a smaller number of subjects we shall get as good results. This is clearly not the case in the continuous subtraction experiment. Obviously it depends on the relation of the variability of the individuals among themselves, on the one hand, to the variability of the measurements on any one individual, on the other. If, for example, the standard deviation of the measurements were the same as that of the individuals the probable errors would be as follows:

Table II.

Probable Errors.

Number of subjects	Number of tests					
	1	2	3	4	5	∞
10	15·1	13·1	12·3	11·9	11·7	10·7
20	11·7	9·2	8·7	8·4	8·3	7·5
100	5·2	4·1	3·9	3·8	3·7	3·4
1000	1·5	1·3	1·2	1·2	1·2	1·1

In such a case it is obviously desirable to make more than one measurement on each subject. Thus, by making five tests on each of ten subjects we get as precise a result as if we had single tests on twenty subjects.

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[1] *Amer. J. of Psychol.* xv. 201–292. *Ztsch. f. Psychol.* 1906, xliv.

The above results point to the following conclusions:

(1) If an accurate value of the racial or class standard deviation is required, and if the variability of the measurements on each individual is high, it would appear that the 'raw' standard deviation should be corrected by subtracting the value of $\sigma_m{}^2/m$ from its square. In all cases, however, it should be remembered that corrections based on the theory of probability are themselves subject to error, are only 'probable' corrections both as regards amount and direction, and if large are certainly untrustworthy, being usable in the latter case only as a touchstone for deciding the rejection of results, not as corrections; while corrections comparable in amount with the probable error of the quantity corrected are unnecessary.

(2) In planning a set of measurements we should if possible obtain a rough measurement of the values of σ and σ_m, and from these decide the number of times it is necessary to repeat the measurements. We should thus ensure that on the one hand we should get the most accurate value from a given number of subjects, and on the other we should be saved the trouble of making more measurements on each subject than are necessary.

In ordinary experimental work in Psychology and Education it is found that σ_m varies from individual to individual, but the mathematical treatment of this case is impossible in view of the lack of knowledge as to how variability is correlated with performance and as to how variability is distributed. It is also possible that the treatment will be different for different kinds of work. Brown, for example, finds that there is a pronounced positive correlation between ability and variability in the case of speed of addition, and a negative correlation in the case of accuracy[1]. In all cases, however, the variability of σ_m will be small relatively to Σ and σ_m, and it is probable that the above results will hold sufficiently closely if σ_m is replaced by the mean value for all the individuals.

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[1] W. Brown, *Essentials of Mental Measurement*, 127.

(*Manuscript received* 15 *January*, 1920.)

THE GENERAL FACTOR FALLACY IN PSYCHOLOGY.

BY GODFREY H. THOMSON,
Armstrong College, Newcastle-upon-Tyne.

I. A natural relationship common to all sets of correlation coefficients is what Prof. Spearman has called hierarchical order.
II. The illusion that the hierarchical order found in psychological research is perfect is due to a mathematical error.
III. The first 'condition' which Mr Garnett would impose on the bonds which cause correlation is as unnecessary as is a General Factor.
IV. So also is Mr Garnett's second 'condition.'
V. Mr Garnett's 'linear transformation' is only completely successful when hierarchical order is perfect, and is in any case only an elaborate way of saying that hierarchical order may be due to a General Factor.
VI. Conclusions.

IT is with regret that I find it necessary, in view of a recent article by Mr J. C. Maxwell Garnett[1], to return to the question of the phenomenon of hierarchical order among correlation coefficients and to insist, with all the emphasis of which my pen is capable, that the deduction of the presence of a General Factor which Professor Spearman has based upon this phenomenon is utterly and entirely invalid.

I.

Hierarchical order is the natural order to be found among any set of correlation coefficients, *however the correlations may be caused*, provided only that the bonds which cause the correlations are left, as we say, to chance: that is, provided the phenomenon is of that complex nature which, in default of analysis, we call *random*. Hierarchical order will arise among correlation coefficients unless we take pains to suppress it. It does not point to the presence of a General Factor, it cannot be made the touchstone for any particular form of hypothesis, for it occurs even if we only make the negative assumption that *we do not know* how the correlations are caused, that is, if we assume only that the connexions are random.

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[1] This *Journal*, 1920, x. 242.

That this is so, follows quite generally from the investigations of Professor Pearson and Professor Filon on the correlation of correlation coefficients, as I have shown elsewhere[1]. If correlation coefficients are arranged in the square table usual in psychological research—an arrangement which Mr Garnett calls a correlation table, though that term has always had a quite other meaning—then the visible expression of the Pearson and Filon formulae is the occurrence of ridges in the pattern of numbers, and of peaks at the nodes where two ridges cross. This means, as is shown in my paper just referred to, that the coefficients can be arranged in hierarchical order. That paper was indeed more general than I then felt justified in claiming: and such expressions of caution as I inserted from an instinct of scientific reserve were in fact unnecessary, as further consideration has shown me.

It is not even requisite that the correlations should be produced by overlapping, that is by the fact that some elements occur in both the correlated variables. They may be caused in more subtle and less tangible ways, of which, as illustration, may be instanced the correlations between hands at whist: and connexions even less easy to visualise can be conceived. All these possibilities are included.

II.

Prof. Spearman's argument is in outline as follows: (1) A considerable degree of hierarchical order is observable among correlation coefficients. (2) After allowance has been made for errors, by means of a certain criterion which we may call 'the Hart and Spearman criterion,' this hierarchical order is found to be perfect, or practically perfect, in every case. (3) Therefore the correlations are caused solely by a General Factor.

The difficulty of scotching this fallacious argument is due largely to the presence of the second step, for many experimenters have been led to believe that the hierarchical order in their results was indeed perfect. But this illusion arises entirely from an error in the Hart and Spearman criterion, which, as I have shown in another paper[2], creates the regularity which it purports to detect. The use of this criterion has vitiated at their very foundations a number of researches many of which are, from an experimental aspect, of great value: it is for example freely employed

[1] *Proc. Roy. Soc.* A, 1919, xcv. 400.

[2] *Biometrika*, 1919, xii. 355.

in Dr Webb's memoir "Character and Intelligence[1]," and is the first formula in Mr Garnett's paper "On Certain Independent Factors in Mental Measurement[2]."

This illusion being cleared away, and the hierarchical order recognised as only imperfect, the fallacy of basing a hypothesis on a phenomenon, which occurs whether the hypothesis be true or not, is evident. We are forbidden by the whole of our scientific training to postulate entities where they are not necessary.

If I make the discovery that the angles of a quadrilateral are equal in sum to four right angles, I may not conclude that it is a square. This is, by analogy, what Prof. Spearman did when he noticed hierarchical order, and deduced a General Factor. True, the angles of a square are equal to four right angles, and the thing may be a square: and similarly a General Factor may exist. But the marks of the genus do not define the species.

Nor may I conclude that I may call it a square because that is simpler: which is, by analogy, what Mr Garnett does. To be exact, he does rather more than that. He admits that it may not be a square, but holds fast to the faith that it is at least rectangular, a conclusion which is as unwarranted as the narrower one. He claims, that is, that while a General Factor may not be present, yet the elements which are present must be bound by a certain Rule[3], and must fulfil two conditions[4]. *These hampering restrictions are just as unnecessary as the General Factor.*

III.

This assertion is already proved by the generality of the work based on the Pearson and Filon formulae. But to make the matter more clear, it may be worth while to give *ad hoc* consideration to these conditions.

Mr Garnett considers that if a hierarchy is to be produced without a general factor, then "any two given higher level elements both of which enter into any test must *tend* to enter in the same ratio (unity or any other) into that test as into any other test into which they both enter." And he protests that I have artificially supplied this condition by making the elements *all-or-none* in their action.

Now in the first place I must claim that the *all-or-none* principle is, in view of recent work in neurological research, not altogether an unlikely one. But I hasten to add that when I was "careful to qualify the conclusion" by means of the sentence asserting the *all-or-none* principle, I did so, not because the conclusion was untrue without such qualification, but because the experiments upon which I had based it in that paper were with *all-or-none* elements, namely dice. If I had omitted the qualification, the conclusion would have been wider than I was justified in drawing from those particular experiments, but it would in point of fact have still been true.

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[1] This *Journal, Monograph Supplement,* 1915.

[2] *Proc. Roy. Soc.* A, 1919, xcvi. 91.

[3] *Op. cit.* p. 248.

[4] Numbered (1) and (2), upper half of p. 255.

For let us abandon for a while in the higher elements the *all-or-none* principle and leave the extent to which each element acts, in each activity into which it enters, *entirely to chance.* In spite of our doing this, the ratio in which two given elements enter any test will still *tend* to be the same as that in which they enter another test of which they form part. I shall refrain from burdening the pages of this *Journal* with the mathematical symbols which would be required to prove this generally. Such a proof would be repellent to the non-mathematical reader, and is unnecessary to the mathematical reader, who can supply it for himself by analogy with the following simple illustration. Let us assume that an element may enter into a test *either* with one-half *or* with the whole of its energy. Consider two elements entering into a certain test. The possible ways in which they may do so are four, namely:

(1) ½ : ½;
(2) ½ : 1;
(3) 1 : ½;
(4) 1 : 1.

That is, in two cases, those numbered (1) and (4), their ratio is unity. In the remaining cases their ratio is ½ and 2 respectively.

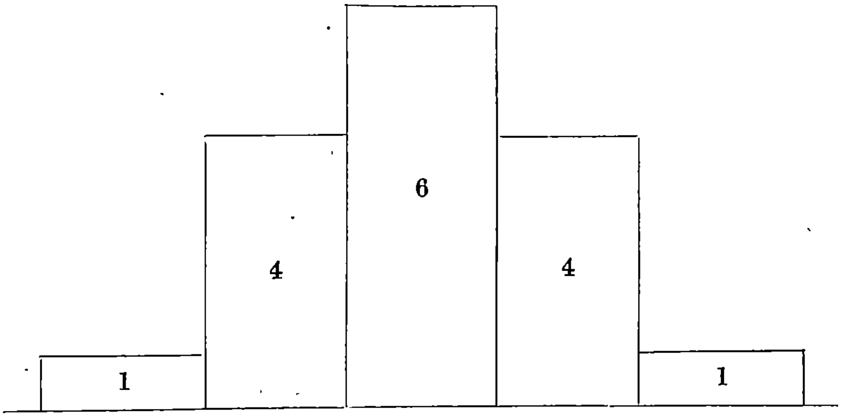
Now consider their entry into a second test or variable, which will take place in the same four ways. Any one of the four ways in which they enter the first test can be associated with any one of the four ways in which they enter the second, making sixteen cases in all. These sixteen cases are shown in the following short table, in which the marginal figures indicate the ratios in which the elements enter the first and second tests or variables respectively, while the inner figures show the relation-

		Ratio in first test			
		½	1	1	2
Ratio in second test	½	1	½	½	¼
	1	2	1	1	½
	1	2	1	1	½
	2	4	2	2	1

ship of these ratios to each other, the second divided by the first. The two ratios are identical in six of the sixteen cases. In detail, the fraction

$$\frac{\text{second ratio}}{\text{first ratio}}$$

takes on the values $\frac{1}{4}$, $\frac{1}{2}$, 1, 2, 4 in the proportions 1, 4, 6, 4, 1 out of sixteen. The last set of numbers will be recognised as the coefficients of a binomial. Expressed graphically, they give the histogram



in which the tendency towards the central value is evident. In the general case this would become a normal curve. The difficulty about the spacing along the x-axis which may occur to some reader may be minimised for him by the reminder that we might just as well have taken the reciprocal ratio.

IV.

Turning to Mr Garnett's second condition, we find that it too is a 'condition' which fulfils itself and need not be made. It is that the elements must combine "so that the number of higher level elements that enter as group factors into any two...tests *tends* always to be proportional to the product of the whole numbers of elements in the two...tests."

Now this is the fundamental theorem in probability; and that they will so tend without further ado is obvious. For what is the chance that a given element will enter a test? It is the whole number of elements in that test, n_1, divided by the entire number of elements in existence, N; it is

$$\frac{n_1}{N}.$$

In a similar way, the chance that it will enter into the second test is

$$\frac{n_2}{N},$$

where n_2 is the whole number of elements in the second test. And the chance of an element being in both is the product

$$\frac{n_1}{N} \times \frac{n_2}{N}.$$

The conditions which Mr Garnett requires to be satisfied are therefore exactly those which mere chance will satisfy. It is as unnecessary to require them as it is to postulate a General Factor.

The additions which Mr Garnett makes to my explanation of hierarchical order are therefore unnecessary and indeed erroneous; as erroneous as saying that a quadrilateral must be rectangular if its angles are to have a sum of four right angles. And indeed his whole placing of the two 'alternatives' (by means of which hierarchical order may be explained[1]) as alternatives of equal standing is erroneous. The relationship between them is not one of alternation but of subalternation. The second is greater than the first, and includes it, as the genus quadrilateral includes the species square.

V.

The paper is, in fact, apart from the errors which I have pointed out, merely an elaborate way of stating that the theory of a General Factor, although not proved by hierarchical order, is yet left as a possible (though unproven) hypothesis. Mr Garnett's last paragraph (before his summary) is headed: "Dr Thomson's results are consistent with the existence of Prof. Spearman's single general factor in every set of sufficiently dissimilar mental qualities." Of course they are. The only objection I have to Mr Garnett's statement of this obvious fact is, that it may very well lead the reader to imagine that I do not realise it. I would refer the reader to the first two sentences of my first paper[2] on the subject: to the sentence on the first page[3] of a later article, in which I say that "there are many theories *in addition to* that of Prof. Spearman which will explain hierarchical order": to a sentence in a third and more general paper[4], in which I say "there is no doubt but that such a general factor, combined with such an absence of group factors, would produce hierarchical order": and to many other places in those articles. A figure whose angles total four right angles may of course very well be a square, but its squareness is not thereby proved, nor is the existence of general ability.

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[1] *Op. cit.* bottom of p. 254 and top of 255.

[2] This *Journal*, 1916, VIII. 271.

[3] This *Journal*, 1919, IX. 337.

[4] *Proc. Roy. Soc.* A, 1919, XCV. 401.

Mr Garnett's way of stating the whole problem is algebraic in form, but no new truths are thereby revealed. He discovers that the *general* equations which express my general explanation of hierarchical order can, by "a linear transformation of independent variables" be written in that *special* form which expresses the theory of a general factor: he discovers that a quadrilateral, if no further conditions are laid down, may be a square.

In passing it may be noted that, in carrying out this 'linear transformation,' Mr Garnett is as elsewhere obsessed by the idea of absolutely perfect hierarchical order. It is only in this case that the transformation is completely successful, and only in this case that group factors entirely disappear and the factors left are either specific, or "small interchangeable parts of a general factor." But such a transformation can in any case, it may at once be agreed, show that such hierarchical order as is present in a practical case may be due to a general factor, though it could not get rid of group factors except in the perfect case.

Of similar origin is Mr Garnett's demand for an infinite number of factors if the general factor is disallowed. One might in reply point to the still larger number of specific factors required in the alternative, an infinity of higher order. But in practice by no means an infinite number of group factors is required. In that one of my dice experiments to which alone Mr Garnett refers in detail, there were employed 145 factors, of which 36 were group factors: and in another purely chance hierarchy described in this *Journal*[1], there were 87 specific and 13 group elements. In a third, described in another place[2], there are 57 specific and 13 group elements, and the hierarchical order, though poorer, is still very considerable, *and is claimed as absolute perfection by the Hart and Spearman criterion*[3]. Finally, if Mr Garnett cares to try a numerical example (and his errors and misconceptions are almost entirely due to his neglect of this salutary practice) he will find that 13 group factors alone, with no specific factors at all, will give quite a reasonable hierarchy[4]. Infinity, therefore, turns out to be in practice about a couple of dozen, implying a degree of complexity which we may surely without hesitation postulate of the human mind or brain. A conservative estimate of the elementary qualities of sensation alone places them at about 50 to 80[5].

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[1] 1919, IX. 341–3.

[2] *Proc. Roy. Soc.* A, 1919, XCV. 401–3.

[3] *Biometrika*, 1919, XII 361–3.

[4] Cf. This *Journal*, 1919, IX. 341, line 21.

[5] W. McDougall, *Physiological Psychology*, London, 1905, 75.

The whole paper raises the question of what writers mean by a General Factor; the question of the degree of reality and individuality which is attributed to factors both general and specific. For consider another illustration. Let us suppose that five subscription lists were opened in a town, and that the sums obtained were £190, £170, £183, £115 and £124. Then, if I do not see the subscription lists, it is open to me to believe *either* (1) that a lot of people have contributed to each list, no doubt some to more than one list, *or* (2) that a beneficent gentleman, acting the part of a general factor, has given £100 to each. But, even if I write out mythical subscription lists on the second theory (even, that is, if I perform a linear transformation), the actual lists are in existence and may well show that no flesh and blood donor of £100 existed.

And in the same way, if we attribute any degree of reality to the elements which enter into any mental test—if, for example, we assume each to correspond to a neurone, or to a synapse, or to a nervous arc—then a 'linear transformation,' though it may give mathematical equations which describe the quantitative situation, yet does so by substituting for (say) the real neurones a set of mythical neurones, of which each is composed of a little bit of this, a little bit of that, and a little bit of the other real neurone.

VI.

I have ventured to assert, more emphatically than in former papers, the utter invalidity of deducing a general factor from hierarchical order unless absolutely perfect. Hierarchical order, of a high degree of perfection, is the natural relationship which *all* correlation coefficients bear to one another. The Hart and Spearman criterion for the degree of perfection is erroneous and creates the extreme perfection it purports to detect, and which, were it really present, would, it has always been admitted, imply a general factor.

Mr Garnett's attempt to restrict my general statement by qualifications breaks down, for the conditions he wishes to impose are those which chance itself supplies.

The general factor theory is of course, as I have always said, possibly true: but it is unproven, as squareness is unproven of a quadrilateral merely by reason of the fact that its angles are, in the sum, equal to four right angles. To prefer the general factor theory because of its simplicity is as likely to lead to error as would the equally simple practice of making all quadrilaterals square in geometry.

(*Manuscript received* 22 *March*, 1920.)

FLUCTUATIONS IN MENTAL EFFICIENCY.

By B. MUSCIO.

(*A Report to the Industrial Fatigue Research Board.*)

I. Aims.
II. The experiments.
  (*a*) *First experiment.*
  (*b*) *Second experiment.*
    (1) *The subjects.*
    (2) *The tests.*
    (3) *Lighting, ventilation and temperature.*
    (4) *Procedure in giving the tests.*
    (5) *Scoring the tests.*
    (6) *The equal groups.*
    (7) *Chronology of the tests.*
III. Results.
IV. Discussion of results.
    (1) *The* 11 *a.m. and* 5 *p.m. results.*
    (2) *Results of the working and resting groups*
V. Conclusions.
VI. Appendix.

I. Aims.

As the character of much of the work in industry is such that it may be supposed to induce central nervous, rather than peripheral muscular, fatigue, it would appear that in seeking reliable tests of industrial fatigue those tests should be selected which are likely to reveal changes in central conditions. Accordingly the tests which form the basis of this paper have been 'mental' tests, and they have been employed to investigate the fatigue caused primarily by continuous demand upon the attention.

II. The Experiments[1].

(*a*) *First experiment.*

In the first experiment, three tests—a calculation test, a group number checking test, and a complex cancellation test (all described below)—were given eight times a day to twenty young women engaged in continuous academic work—attendance at lectures, laboratory work and scientific reading. The first test on each day began at 10 a.m. and the last at 5 p.m., the others being distributed over the intervening time at intervals of one hour. On each occasion the tests lasted only a few minutes, and they were carried on for five days. The data showed one result conclusively: that *experiments of this kind are absolutely valueless for the investigation of fatigue because of the complications caused by practice.* It is impossible to determine whether fatigue causes a deterioration in the performance of any test so long as practice is causing an appreciable improvement in its performance; and in the present case the subjects were still improving at the end of the fifth day. In such a situation the opposing influences of practice and fatigue cannot be satisfactorily evaluated.

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[1] I wish to thank Dr Winifred Cullis for making all arrangements with her students for the carrying out of these experiments, and the students themselves for taking part in them; also Dr C. S. Myers for very kindly reading through the MS. of the paper and making valuable suggestions.

The first problem therefore was to obtain a satisfactory experimental method by which the effects of fatigue could be separated from the effects of practice. Now, it is *in theory* always possible to eliminate practice by prolonging an experiment until practice effects become inappreciable; but such procedure is hardly ever feasible because of the demands it makes upon subjects. In giving tests to industrial operatives this difficulty is greatly increased; for they themselves have little scientific interest in the investigations, and the industrial manager will not readily consent to their giving much time to the working of tests. Is there, then, any other method by which practice can be eliminated?

Such a method is the 'method of equal groups.' This consists in first obtaining groups of subjects equal in number and in the capacity for a given test, and in then applying the test to these groups under any desired conditions. The simplest case is that in which there are only two such equal groups. These are obtained by first giving the test to all the subjects who are to be tested and by dividing them into two equal groups on the results. One group is then given the test, say, before work, and the other, say, after work. If the groups are known to be equal in capacity for the test, any difference in the results then obtained from them may be considered a probable fatigue effect. At any rate, it could not be due to practice, as the two groups would be at an identical stage of practice in the hypothetical condition stated. The method of equal groups was therefore made the basis of the experiments now to be described.

(b) *Second experiment.*

(1) *The Subjects.* These were thirty-four third year students (average age = 22 years, *m. v.* = 2·9) at the London (Royal Free Hospital) School of Medicine for Women. Sixteen of these students had already been subjects in the first experiment.

(2) *The Tests.* Three tests, which will be referred to as T_1, T_2, and T_3, were used. These were the same tests that were used in the first experiment, and consequently the sixteen students who had participated in that experiment were in a fairly advanced practice stage with regard to them. To the remaining eighteen students the tests were new.

T_1 consisted of 40 arithmetical examples of the following type: $6 \times 5 \div 3 = 11, 15, 30, 10, 20$. The correct answer was included among the numbers on the right-hand side of the equation, and had to be underlined.

T_2 consisted of 8 vertical columns of 6-place numbers. The subjects were required to underline each of these numbers containing a 1 and 3 and 7. Each vertical column contained 25 6-place numbers, and 10 of these contained all three specified digits.

T_3 consisted of 20 horizontal rows of 30 digits each. Subjects were required to put a vertical stroke through every 3, a horizontal stroke through every 6, and a circle round every 5. There were 10 digits to be marked in each row.

The tests were always worked in the order T_1, T_2, T_3.

Eight different test papers (blanks) were used for each test. Once the subjects had worked through eight test periods, they had to use again test papers containing exactly the same figures as had occurred in the earlier papers.

(3) *Lighting, Ventilation and Temperature.* The lighting and ventilation of the room in which the tests were carried out were good and seemed very constant. The range of variation of the room temperature during the experiments was not more than 2·5° C.

(4) *Procedure in Giving the Tests.* (*a*) *Test periods.* A test period lasted four minutes: $\frac{3}{4}'$ was allowed for T_1, $1\frac{1}{4}'$ for T_2, and $2'$ for T_3.

(*b*) *Giving the Tests.* At the word 'go,' the subjects, who were seated at tables, pencils in hand ready to begin, started T_1; at the end of $\frac{3}{4}'$, the word 'next' was used as a signal to proceed to T_2; at the end of another $1\frac{1}{4}'$ the word 'next' was again used as a signal to proceed to T_3; and after a further $2'$, at the word 'stop,' the subjects immediately ceased work and left the testing room. The tests were arranged so that they could not be seen until a page was turned, which was only done when the word 'go' was given.

Before working the tests at any period, the subjects stated on the cover of the test papers: (1) how they had spent the preceding hour; (2) their feeling of relative fitness (in four degrees); (3) any obvious factor, such as toothache, which might interfere with the results.

(c) *Instructions given to the Subjects.* These were general and special.

The *general* instructions were the two following:

α. They were to do their very best at every test period, no matter how tired they might feel.

β. They must observe absolute secrecy with regard to the tests until the whole experiment was over.

The *special* instructions concerned the methods to be followed in working the tests. Thus, the specified digits in T_3 were to be marked as the subjects reached them in proceeding *along* the rows: the subjects were not to mark all the fives in one row first, for instance, then all the threes in that row, and then the sixes. Mistakes were not to be corrected. Finally, the work had to be done as rapidly and accurately as possible.

(5) *Scoring the Tests.* As the results presented below are those for T_3 (almost entirely), it is not necessary to refer to the scoring of the other two tests. In T_3 each digit (presumably) inspected up to the last marked was counted as one unit and included in the results for quantity. The justification for this was that each horizontal row contained the same number of digits to be marked.

(6) *The Equal Groups.* The subjects were divided into two sets (A and B) and each of these was divided into two equal groups (A_1, A_2 and B_1, B_2). The A set consisted of the sixteen subjects who had taken part in the earlier experiment, while the new subjects constituted the B set. This division into sets was made largely because of the two different practice stages of the subjects in the tests. The A set was divided into two equal groups (A_1 and A_2) *on the total results for T_3 obtained on the fourth day of the first experiment*—the fifth (final) day would have been chosen but that one of the 16 students was absent on that day. Each A subject was thus given a position in a sub-group on the results of 8 separate test-periods at a fairly advanced stage of practice. The ranking of these subjects has therefore considerable reliability.

The B set was divided into two equal groups (B_1 and B_2) *on the results for T_3 of four separate test periods* (which had been preceded by two 'adjustment' periods given to familiarise the subjects with the tests).

The reason for using only one of the three tests in obtaining the equal groups was that no satisfactory combination of them could be effected. Owing to individual differences, two groups which were equal in capacity for any one of the tests were not equal in capacity for any other. It was therefore necessary to limit the investigations to one of the tests, and T_3 was chosen. Consequently, it is the results obtained from the equal groups for this test that are important, for it was only with regard to the capacity for this test that the groups were equal. It should be remembered, however, that the other tests were always worked at each test period before T_3.

The equality of A_1 with A_2, and of B_1 with B_2, is indicated by the figures in the following Table (I) which represent the *quantities* of work in test units[1] done by the various subjects in T_3. In obtaining the equal groups no account was taken of errors: that is, the equality of the groups was based merely upon quantity, not quality, of test output. The justification for this is that in the first experiment the errors in this test were extremely few (they varied between 0·53 and 0·68 of 1 per cent.[2] of the quantity of work).

TABLE I.

Equality of equal groups for T_3.

A_1		A_2		B_1		B_2	
Reference No. of Subject	Average quantity in T_3 (8 test periods)	Reference No. of Subject	Average quantity in T_3 (8 test periods)	Reference No. of Subject	Average quantity in T_3 (4 test periods)	Reference No. of Subject	Average quantity in T_3 (4 test periods)
1	564	9	548	17	377·5	26	402
2	523	10	542	18	368	27	344·5
3	515	11	520	19	342	28	333
4	502	12	503	20	329	29	327·5
5	481	13	470	21	327	30	322·5
6	465	14	460	22	318	31	322
7	450	15	436	23	307	32	315·75
8	389	16	402	24	305	33	305
				25	292·5	34	293·5
8 S's = 3889		8 S's = 3881		9 S's = 2966		9 S's = 2965·75	

(7) *Chronology of the Tests.* The chronology of this experiment was as follows. It lasted for five days (Nov. 3rd to 7th[3] incl.). On Nov. 3rd, the

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[1] A test unit in this test = one digit in the blank. As each horizontal row contained 30 digits, a subject who had worked through 15 rows, for instance, would have done 450 test units of work. See this section, paragraphs (2) and (5).

[2] The errors here are the total errors for all the subjects at the respective hours, and similarly the quantity of work is the total work of all the subjects at the same hours.

[3] 1919.

B set was given the six preliminary tests referred to above (6); and the *A* set one readjustment test (to re-familiarise the subjects with the tests).

On the 4th, A_1 and B_1 were given the test at 11 a.m., and A_2 and B_2 the (same) test at 5 p.m. On the 5th A_2 and B_2 were given the test at 11 a.m. and A_1 and B_1 at 5 p.m. (Reversing the groups in this way was designed to prevent the occurrence of any difference between morning and afternoon results due to an unrevealed inequality between the 'equal' groups.) On the 6th and 7th, the *A* set continued this procedure: on the 6th, A_2 took the test in the morning and A_1 in the afternoon; and on the 7th, A_1 in the morning and A_2 in the afternoon. On these two days the *B* set followed a quite different procedure. One of the *B* groups rested ('did nothing') while the other continued its usual academic work on the first of these days, and on the second the group that had rested on the first day worked, and that which had worked rested; and during these days, both groups of the *B* set were given the tests 8 times a day (the times were the same as for the *A* set in the first experiment). The chronology of the test periods is set out in the following Table (II).

TABLE II.

Chronology of Test Periods.

Date	10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	Remarks
Nov. 3rd	—	—	*Bx*	*Bx*	*By*	*By*	*Ax* *By*	*By*	*Ax* = Readjustment test for *A* set *Bx* = Adjustment tests for *B* set *By* = Tests for obtaining equal groups for *B* set
4th	—	A_1	—	—	—	—	—	A_2	
,,		B_1						B_2	
5th	—	A_2	—	—	—	—	—	A_1	
,,		B_2						B_1	
6th		A_2						A_1	
,,	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	(B_2 working, B_1 resting)
7th		A_1						A_2	
,,	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	B_1B_2	(B_1 working, B_2 resting)

The 'doing nothing' consisted of various things, such as sitting about, chatting, taking 'gentle walks,' reading light literature, playing slight parlour games. 'Work' was definitely put aside, and no activity was entered upon seriously, but merely as a way of putting in time.

III. Results.

The results are shown in the following table and figures. Table III shows the quantities of work done by the equal groups at 11 a.m. and 5 p.m. The figures given are the group averages in test units. In the last vertical column, the afternoon results are shown as percentages of those obtained in the morning. In Table IV are given the T_3 results for the working and resting groups (B_1 and B_2 on Nov. 6th and 7th). Here, again, the figures are the group averages at the various hours. Graphical representation of these results (both Tables) is shown in Figs. 1 and 2.

Table III.

The 11 *a.m and* 5 *p.m. Results.*

Date	11 a.m (T_3) group average and standard deviation (*s. d.*)	5 p.m (T_3) group average and standard deviation (*s d.*)	Difference between a.m and p m results (in test units)	P.E. of difference	5 p m. results as percentage of 11 a.m. results (T_3)
Nov. 4th	A_1=481·1 *s. d.* A_1= *69·4*	A_2=499·5 *s.d.* A_2= *53·2*	18·4	7·4	in *A* groups=103·8
	B_1=377·7 *s. d.* B_1= *36·3*	B_2=391·0 *s. d.* B_2= *29·6*	13·3	3·5	in *B* groups=103·5
5th	A_2=538·5 *s. d.* A_2= *46·3*	A_1=503·0 *s. d.* A_1= *63·0*	35·5	6·6	in *A* groups= 93·4
,,	B_2=411·3 *s. d.* B_2= *20·6*	B_1=387·3 *s. d.* B_1= *40·3*	24·0	3·4	in *B* groups= 94·2
6th	A_2=556·6 *s. d.* = *61·2*	A_1=524·8 *s. d.* = *61·6*	31·8	7·0	in *A* groups= 94·3
7th	A_1=540·5 *s. d.* = *58·1*	A_2=533·5 *s. d.* = *38·6*	7·0	5·9	in *A* groups= 98·7

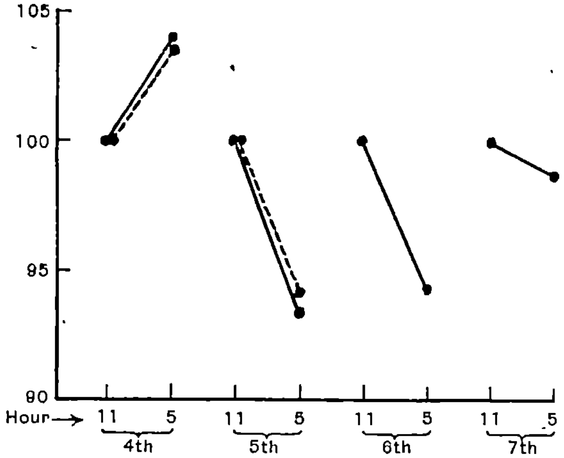

Fig. 1. The dots joined by broken lines are the *B* groups, those joined by solid lines the *A* groups. Ordinates indicate group averages for quantity. These are shown in percentages, as given in the right-hand column of Table III.

TABLE IV.

Results for Working and Resting Groups.

The figures show the group averages and standard deviations (*italics*) in test units.

Date	Group	Hour of Day								
		10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	
Nov. 6th	B_1	431·7	430·9	435·7	450·1	451·2	437·6	433·9	437·4	B_1 Resting
		32·52	*45·64*	*49·48*	*56·77*	*42·56*	*25·48*	*43·37*	*36·71*	
,,	B_2	425·4	422·5	432·5	436·4	441·3	422·7	419·5	428·8	B_2 Working
		31·37	*31·91*	*33·29*	*29·61*	*38·79*	*43·80*	*37·06*	*34·65*	
Nov. 7th	B_2	462·4	478·9	483·1	485·2	471·2	467·3	471·9	500·1	B_2 Resting
		36·40	*40·67*	*38·60*	*41·90*	*43·78*	*31·10*	*16·37*	*19·42*	
,,	B_1	454·0	464·7	465·9	472·5	469·7	461·5	464·3	472·2	B_1 Working
		46·06	*25·75*	*50·45*	*43·91*	*57·19*	*44·57*	*24·98*	*41·93*	

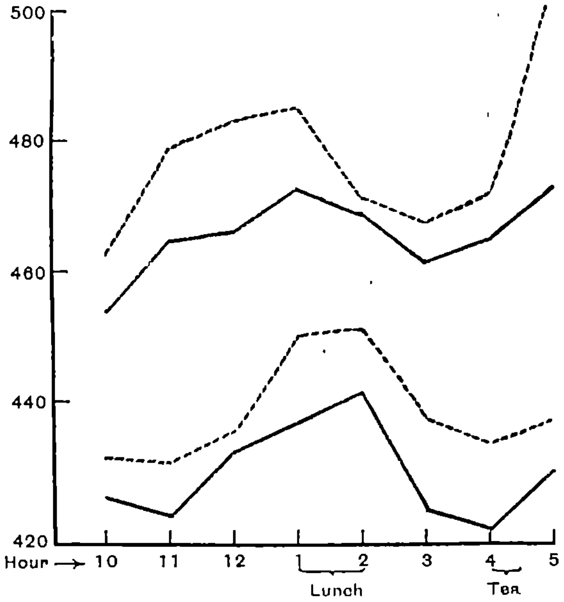

Fig. 2. The broken lines are the resting groups, the solid lines the working groups. Ordinates indicate group averages (Table IV). The two upper curves refer to Nov. 7th, the two lower curves to Nov. 6th. (Practice accounts for the fact that the curves for the 7th are above those for the 6th.)

IV. Discussion of Results.

(1) *The* 11 *a.m. and* 5 *p.m. results*. These show a smaller output in the afternoon than in the morning on three out of the four experimental days, though on one of the three (Nov. 7th) the difference is very small. On the remaining day (Nov. 4th) the smaller output belongs to the morning. In most cases the differences are statistically significant. On Nov. 5th the difference for the A groups is more than five times, and for the B groups more than seven times, its 'probable error.' In two cases (the A groups on the 4th and 7th) the differences are less than three times their P.E.'s; but the defect from this standard is not great on the 4th, and the circumstances of the 7th were somewhat exceptional, as will be pointed out presently.

These differences between the morning and afternoon output seem definitely *not* to have been due to certain factors. Thus, (1) they were not due to *practice* (on Nov. 4th), as the afternoon groups (on that day) were in exactly the same practice stage as the morning groups. (2) Since the subjects were unusually trustworthy and were instructed to do their utmost on every occasion, the differences were not due to *differences in volitional effort*. (3) *Inequality between the morning and afternoon groups in capacity for the test* is ruled out as an explanation by the method adopted in obtaining the groups. Further, (4) as the records showed that on *every* day *the feeling of weariness* was greater in the afternoon than in the morning, the differences (on Nov. 4th, at least, and therefore possibly on the later days) were not due to differences in the intensity of this feeling. (5) As to *environmental conditions*, the variations in these seemed to be very small; and it has been shown that within certain wide limits such conditions as temperature and humidity do not appreciably affect work when subjects are definitely trying to do their best[1].

Were the differences due to fatigue? If we accept the usual interpretation of output figures and suppose that they were, we have to explain the results on Nov. 4th, when the larger output was in the afternoon. Do these results indicate that some factor interfered with the effect of fatigue on this day, and that tests of the nature of T_3 are therefore of no value for fatigue investigation?

The similarity of the results obtained from the A and B groups on Nov. 4th is very great[2], although the A groups were then at a practice stage far in advance of the *B* groups. If some factor interfered with the effects of fatigue on that day, it evidently affected both pairs of groups in the same way (the members of both pairs were doing identical academic work). Investigation elicited the fact that on the morning of the 4th, during the hour immediately preceding the 11 a.m. tests (*i.e.*, from 10 a.m. to 11 a.m.), the subjects attended a lecture which they considered the hardest work of the week[1]. It is therefore possible that on this day the subjects were especially fatigued at 11 a.m. and had partly recovered at 5 p.m. If so, this day's results strengthen the suggestion that T_3, in these conditions, is an indicator of fatigue. If not, the difference in output must here, and perhaps on the other days also, be attributed to unknown factors.

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[1] (1) Thorndike, McCall and Chapman, *Ventilation in Relation to Mental Work*, 1916; (2) L. L. Stecher, *The Effect of Humidity on General Efficiency*, 1916.

[2] See Fig. 1.

Although practice could not have affected the results of Nov. 4th, it *may* have reduced the difference in output of the morning and afternoon groups on Nov. 7th. These groups had each had three test periods (which would act as further practice periods) during the three days immediately preceding Nov. 7th. Each group had had one afternoon and one morning period; but one had had its third period in the morning, the other in the afternoon. On the 7th, the former group took the test at 5 p.m. If fatigue were operative at all, the morning test periods may have had a greater practice effect than the afternoon periods, and hence the afternoon group on Nov. 7th may have been slightly favoured. Had there not been this difference in the times of the test periods, the difference between the output of the morning and afternoon groups on the 7th might well have been greater than it was: the morning group might have done more than it did, and the afternoon group less.

While these results suggest that, for certain types of work, a test similar to T_3 would show fatigue, they do not allow us to deduce this with certainty, because we have no certain knowledge as to the degree of fatigue present at 11 a.m. and 5 p.m. on the four experimental days. The results thus emphasize a constant difficulty, and demonstrate that *the worth of any suggested fatigue test can only be determined if the degree of fatigue present when it was applied is known independently of the test results.*

(2) *Results of the Working and Resting Groups.* The effects of work are shown in the relatively low output of the working group at every hour of the day. The actual differences in output between the working and resting groups at each hour (in test units) together with their probable errors are given in Table V.

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[1] There was a difficult lecture from 9 a.m. to 10 a.m. also on this day.

TABLE V.

Actual differences in test units between the working and resting groups at the various hours on Nov. 6th and 7th, together with the probable errors of these differences. (Actual differences are in favour of the resting group at every hour on each day.)

Date		Hour								Remarks
		10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.	
Nov. 6th	Actual Diff.	6·3	8·4	3·2	13·7	9·9	14·9	14·4	8·6	*Mean Diff.* = 9·9
,,	P.E. of Diff.	2·78	4·15	4·47	4·76	4·31	3·81	4·26	3·75	*Mean P.E.* = 4·04
Nov. 7th	Actual Diff.	8·4	14·2	17·2	12·7	1·5	5·8	7·6	27·9	*Mean Diff.* = 12
,,	P.E. of Diff.	4·36	3·53	4·77	4·52	5·38	4·03	2·23	3·43	*Mean P.E.* = 4·03

The size of the actual difference relative to that of its probable error would render it significant at most hours on each day, even if the results at any one hour were regarded without reference to the results at other hours. But it will be seen that the *mean* actual difference is approximately two-and-a-half-times its probable error on Nov. 6th and three times its probable error on Nov. 7th. Further, when it is recalled that the rôles of the groups were reversed on these two days, and that the superior group while resting was the inferior group while working, the evidence is unquestionable that at least a certain kind of mental work has a deteriorating effect upon the capacity to do mental work similar to that involved in test T_3 (compared with the efficiency of the 'resting' condition).

But perhaps the most important feature in these results is the similarity in the general shape of the curves for the two groups on each day. The rises and falls correspond precisely: there are falls in the curves for the resting groups just as there are in those for the working groups. Are these rises and falls significant? If the curve for either group (on either day) is analysed without reference to the curve for the other group, its variations from its own mean value are so small as to be of little significance: that is, had we only the curve for one of the groups, we could infer little if anything from its rises and falls, since these are on the whole within the range of chance variations. Taking the two curves (for either day) together, however, we get an entirely different situation. The similarity between them, apparent to observation, may be expressed precisely by their correlation coefficient. This is very high. On Nov. 6th, the r of the two sets of results[1] is $+\cdot 88$ ($p.\ e. = \cdot 052$), and on Nov. 7th, it is + ·90 (*p. e.* = ·045). There is thus no doubt about the significance of the rises and falls in the curves, and the only question is how they are to be interpreted. The rises may be passed over briefly, as practice would account for them; we may assume that they represent simply a condition in which practice effects were not inhibited. The important features are the falls. Such falls are usually explained by fatigue; but *as they occur in the curves for the resting as well as in those for the working groups,* this explanation does not seem possible here. They indicate that some factor simultaneously neutralised, or more than neutralised, the practice effects in the two groups, and then gradually tended to lose its inhibitive power. This factor does not seem to have been the feeling of weariness, since the curves for this and output in the test do not correspond; apparently it was not a diminution of volitional effort; and it was not temperature nor humidity[1].

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[1] r = the Pearson coefficient.

Were the falls caused by the taking of food? This seems a probable explanation in view of the altered metabolism which the taking of food would involve. If correct, some special explanation must be found for the Nov. 6th results (Fig. 2) which show the largest output for the day in the first test period after lunch. Possibly the character of the lunch on Nov. 6th was different from what it was on Nov. 7th[2]. At any rate, in the earlier experiment a relatively low output immediately after lunch was the rule, so that there probably is some special explanation of the result on Nov. 6th.

One difficulty in explaining the falls by the taking of food is that the output (in all the experiments) tended *to remain low* during the whole afternoon—notwithstanding some slow recovery. Could the whole of this effect have been produced by the lunch? To the suggestion that fatigue helped to keep the output low in the later afternoon hours, it may be replied that the low afternoon output was on the whole characteristic of the resting group as well as of the working group. Consequently, if fatigue were the factor operative here, it was *not* fatigue *caused by 'work.'* There is at least one other difficulty about the taking of food as an explanation. On Nov. 6th there was a slight fall in the curves from 10 a.m. to 11 a.m. (Fig. 2). Here the suggestion that either the taking of food or fatigue was the cause of the fall seems improbable: for breakfast had been taken probably three hours before the 11 a.m. test on this day (and on Nov. 7th too, when there is a definite *rise* from 10 a.m. to 11 a.m.).

While, therefore, the falls under discussion may have been largely due to the taking of food, this cannot be maintained with any assurance. It seems best at present to say that the causes of these falls are not known.

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[1] There was a difficult lecture from 9 a.m. to 10 a.m. also on this day.

[2] This has been suggested by Prof. E. L. Collis, M.D.

These results thus seem to prove, not merely that a relatively low output need not indicate fatigue, but that it may not indicate any of those factors now accepted as sporadic determinants of a performance, such as a relatively small volitional effort or an intense feeling of weariness. The effects of fatigue appear to be superimposed on those of variations in a 'work mechanism,' the efficiency of which varies in a characteristic manner whether we work or rest.

This result supplements Hollingworth's findings[1] and leads to some modification of his conclusions. On the basis of his results, obtained from thoroughly practised subjects, he concludes that such variations as occur in the efficiency of the 'work mechanism' are not due to diurnal or to organic rhythms (since they occur whether a day's work begins at 7.30 a.m. or 10 a.m.) but to work. The results here presented indicate that these variations occur *even in the absence of work*. It would be important to investigate (with special subjects) their precise nature for different human capacities (Hollingworth found that the work mechanism for motor capacity showed different changes from that for mental capacity) and also, if possible, their determinants.

Whatever be its cause, there is strong evidence for the presence of a diurnal or organic rhythm in the foregoing results.

If we neglect the 10 o'clock results on Nov. 6th and the 5 o'clock results on Nov. 7th, we get curves, covering 6 hours for both days, almost identical in shape. They do not refer to the same six hours for the two days: the curves for the 6th refer to the time from 11 a.m. to 5 p.m., those for the 7th to the time from 10 a.m. to 4 p.m. This strongly suggests the operation of *some* rhythm—earlier by an hour on the 7th than on the 6th.

Results similar to these were also obtained in the first experiment, though here there was no control group of resting subjects: that is, in the first experiment, as well as in the second, *working* subjects yielded differently-shaped curves (for the same test) on different days. It is a fair inference from the results shown in Fig. 2 that the curves of *these working* subjects are typical of the curves which would have been obtained on the same days from *resting* subjects. It would therefore be of interest to make tests over the whole twenty-four hours, or at least over a much larger number of hours than was covered in these experiments.

[1] "Variations in Efficiency during the Working Day," *Psychol. Rev.* 1914, XXI. 473–491.

Some importance attaches to the results for T_1 and T_2 on Nov. 6th and 7th. As the working and resting groups were not equal in capacity for either of these tests, the curves for the resting group are not constantly higher than those for the working group (as they are for T_3, with regard to the capacity for which the groups were equal). The relative heights of these curves at any hour should, therefore, not be considered of any particular significance. The significant feature is general similarity in form over the whole day. The curves are shown in Fig. 3. It is at once obvious that they strengthen the idea that characteristic variations in efficiency[1] occur during a day in addition to variations produced by fatigue *due to 'work.'* And this idea gains still further probability from the following facts.

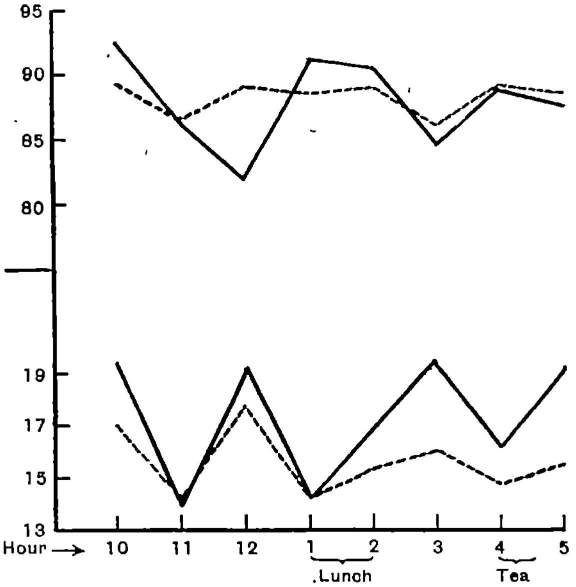

Fig. 3. The broken lines are the resting groups, the solid lines the working groups. Ordinates indicate group averages. The upper curves are for T_2 on Nov. 6th, the lower curves for T_1 on the same day.

In order to test the reliability of the group average curves obtained in the first experiment, the total group of twenty subjects (all working) was divided (after the experiment) into two chance groups of ten each, and the average results of these chance groups were compared. The chance groups were obtained by arranging the subjects in alphabetical order, and taking the first ten as one chance group and the second ten as the other. Samples of the curves thus obtained are shown in Fig. 4; X is one chance group and Z the other.

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[1] These variations would be different for different capacities: at least, this is suggested by such differences as those between the T_1 curves on Nov. 6th (Fig. 3) and the T_3 curves for the same day (Fig. 2).

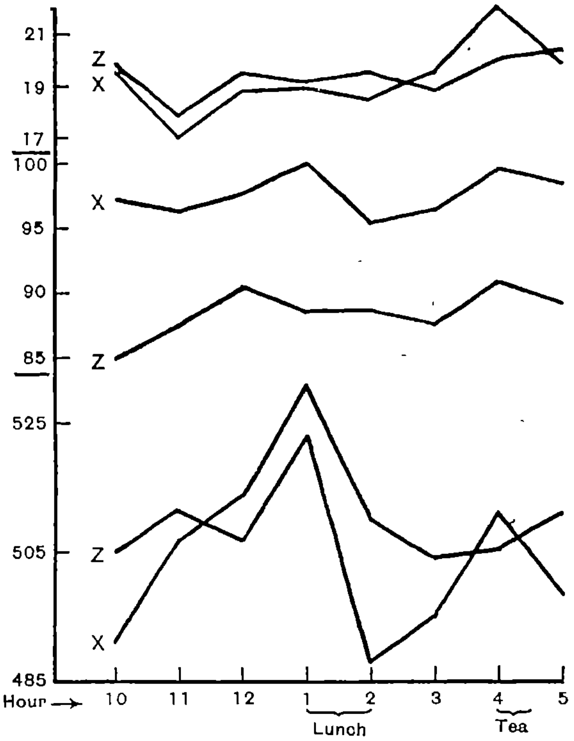

Fig. 4. X and Z = the chance groups in the earlier experiment. The upper pair of curves are for T_1 (June 27th), the middle pair are for T_2 (June 26th), and the lower pair are for T_3 (June 27th). Ordinates indicate group averages.

These curves are very similar for any given test on any given day. The observed similarity may be given precise numerical expression by means of correlation coefficients between the chance groups, and these are always positive and generally high. Thus, the r for the chance groups for T_1 on June 27th (Fig. 4) is + ·76 (*p. e.* = ·10), for T_2 on June 26th (Fig. 4) it is + ·70 (*p. e.* = ·12), for T_3 on June 27th (Fig. 4) it is + ·53 (*p. e.* = ·13). Now, the members of these chance groups were all engaged upon work during the day, and further upon the same work. The similarity of their average curves is therefore not surprising. But—what is somewhat surprising—the curves for the *resting and* *working groups* for the tests in which they were *not equal* are, on Nov. 6th and 7th, also very similar. Thus, the curves for T_1 on Nov. 6th (Fig. 3), almost as similar as those for T_3 on Nov. 6th and 7th (Fig. 2), are *more* similar than any pair of curves obtained from the chance groups in the earlier experiment. The r for these T_1 results is $+ \cdot 81$ ($p. e. = \cdot 083$), whereas the highest r for the chance groups was $+ \cdot 76$ ($p. e. = \cdot 10$). And *in general* the curves of Fig. 3—not to mention those of Fig. 2—*representing working and resting conditions* are quite as similar as the pairs of curves in Fig. 4, which represent the test output of two chance groups *both working*.

A further question of some interest is whether the effects of work gradually became greater as the day advanced—whether fatigue was progressive. Although the appearance of the curves (Fig. 2) does not make the occurrence of such an increase in the effects of work obvious, analysis of the actual figures tends on the whole to support it. On Nov. 7th, it is true, the mean afternoon difference between the two groups is actually less than the mean morning difference; but if the results for Nov. 6th and 7th are taken together, then the mean afternoon difference for the two days is greater than the mean morning difference, though by a small amount only. More important, perhaps, is the fact that if the results for the two days are taken together, the mean difference between the groups for the last two hours of both morning and afternoon is greater than the mean difference for the first two hours: that is, the effects of work seem to be greater in the late stages than in the early stages of a work period. This is in accord with the current view; but it is worth noting that the results for the afternoon of one of the two days (Nov. 6th) are not in accord with this view, and also, as was stated above, that the mean afternoon difference between the groups on Nov. 7th is actually less than the mean morning difference.

On the whole, therefore, these results afford no very convincing evidence that the effects of fatigue, as judged by diminished capacity for work, are progressive throughout a day's work.

Finally, there is a practical consideration. If fatigue was the cause of the differences between the 11 a.m. and 5 p.m. results (Fig. 1), it must have been operative in reducing the output of the *working* group on the afternoons of Nov. 6th and 7th (Fig. 2). But since the curves for the resting groups are, on these days, practically identical in shape with those of the working groups, we may reasonably infer that had *control* (resting) subjects been used in connexion with the 11 a.m. and 5 p.m. results, they would have shown similar differences to the working subjects; that such control subjects would have done *more* than the working subjects both at 11 a.m. and 5 p.m., but that, as with the working subjects, their 5 p.m. output would generally have been *smaller* than their 11 a.m. output. Either, then, such differences are not caused by fatigue, or, if they are, we cannot argue from them as to the relative strenuousness of a subject's work. That is to say, it must be futile to compare the test output of subjects at one time with their test output at another time[1] if we wish to obtain any indication of the hardness of their work, as there will be, within limits, a similar difference in output whether they work or rest. What seems to be necessary is to compare the test output of *working* subjects at any given time with that of *resting* subjects *at the same time.*

V. Conclusions.

1. Possibly as a consequence of the production of fatigue, continuous mental activity, such as is involved in academic study, definitely lowers the capacity for certain mental tests (type T_3, at least).

2. It is impossible to estimate accurately the value, as fatigue tests, of the tests used in these experiments, because positive knowledge of the degree of fatigue present when the tests were applied was unobtainable. There is no doubt, however, that with reliable subjects such tests may yield highly important information (see 3).

3. Within the meanings of 'work' and 'rest' relevant to the conditions of these experiments, similar variations in the capacity for the performance of a task may occur whether a person works or rests; and these variations are probably different for different capacities. This conclusion is especially pertinent to the interpretation of output and accident curves.

4. Practice must be eliminated before any performance test can be used to diagnose fatigue. For this purpose, the method of equal groups is probably the best, and can readily be used in the office or factory.

5. It is essential to make use of control subjects in fatigue experiments. The contrary procedure, which is universal, may lead to fundamentally misleading conclusions—for instance, in the interpretation of the falls in output curves.

VI. Appendix.

I am indebted to Miss S. C. M. Sowton for the following results. Miss Sowton first worked through T_3 145 times (preceded on each occasion by T_2), the test periods being distributed over seven days. By the end of this time practice effects, though not absent, were relatively small. The test was then worked for a further three days (21 times a day at intervals of about half-an-hour, and always preceded by T_2) during which no work whatever was done in the intervals between the tests. The composite curve for these three days is here given. Miss Sowton worked through the whole test blank at each period; and efficiency is therefore measured by the relative smallness of the time taken for the test. In the figure, the ordinates are the times required for working T_3, the abscissæ the hours at which the test was worked (the intermediate points on the curve represent the results of tests at intermediate times).

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[1] On the same day.

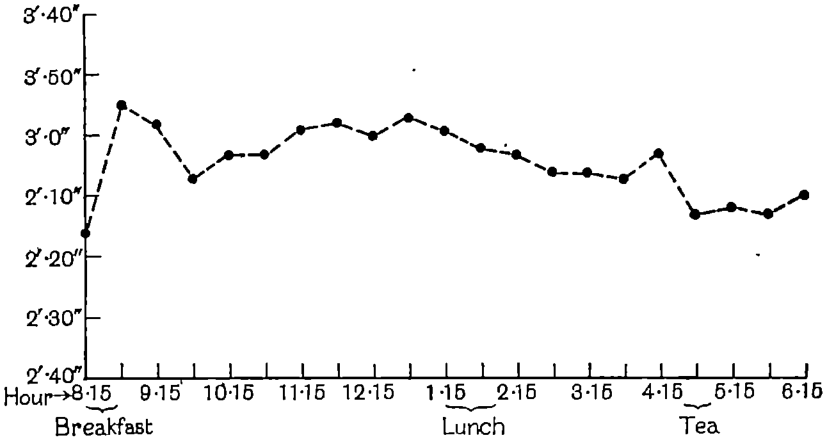

Fig. 5.

It will be seen that efficiency for the test gradually becomes lower during the day. Notwithstanding that the only work on these days was that involved in the doing of the tests,—which amounted to not more than 7 minutes per half-hour,—the test results from 4.15 p.m. onwards are definitely poorer than any other test results during the day with the exception of the first. This tends to confirm the position stated in the last paragraph of Section IV above.

(*Manuscript received* 18 *March*, 1920.)

PUBLICATIONS RECENTLY RECEIVED

Aesthetics: A Critical Theory of Art. By Professor HENRY G. HARTMAN. Columbus, Ohio: H. G. Adams and Co. 1919. pp. 250.

In this book an attempt is made to determine the elements of interest in painting, music and poetry. The author maintains that to attack questions of aesthetics from a purely general standpoint is misleading, and in strictness the book should be called "a critical description of certain of 'the arts.'" Thus all of the first part of the treatise is really introductory to the detailed study of painting, poetry and music which occupies the last three chapters. It is throughout urged that questions concerning the 'substance' of an art—a conception which is nowhere very clearly defined—its technique, conventions and material, deserve equal consideration with problems of individual expression and enjoyment. Consequently psychological aesthetics receives from the author much criticism, of which some is good, and most is interesting. The book is written with the most complete confidence imaginable; but it is both readable, and, for the greater part, worth reading.

Lehrbuch der experimentellen Psychologie. By Professor J. FRÖBES, S. J. Freiburg im Bresgau: Herdersche Verlagshandlung. 1917. Vol. I. pp. xxvii + 605. 9*s.*: bound 10*s.* 9*d.*

The author, now professor at a theological college, learnt his experimental psychology in G. E. Müller's laboratory at Göttingen. The first volume of his text-book imitates his master's work in thoroughness of method and conscientiousness to detail, but it is lacking in originality and system. It consists of a patch-work summary of the work of recent investigators, whose names are mentioned on every page, but generally without reference to their published papers, and without adequate recognition of British and American contributions.

Report of the Care and Control of the Mentally Defective and Feeble-Minded in Ontario. By the Hon. F. E. HODGINS. Toronto: A. T. Wilgress. 1919. pp. 236.

The writer, a Justice of Appeal, was appointed by Royal Commission to consider and inquire into the existing methods of dealing with imbecile, feeble-minded, or mentally defective persons in the Province of Ontario, and to make recommendations in regard thereto. The report contains not only a survey of the present conditions in Ontario and a scheme of proposals, but also an interesting account of the work carried on in the United States and in this country, and reprints of a number of papers by T. W. Salmon, Adolf Meyer and others on mental deficiency in an appendix.

Two New Papers on Hypnotism. By PATRICK P. DALTON, of University College, Galway. Privately printed, N.D. pp. 12. 1*s.* 7½*d.* post free.

Of the two papers contained in this pamphlet, the first is an address to the Galway Medical Faculty on the value of hypnotism as a therapeutic agent. The author rightly distinguishes two methods of employing hypnosis, (*a*) for direct suggestion, (*b*) for the exploration of repressed experience. The second paper is so different in character that it should not have been issued with the first. It is entitled "Investigations into the state of the brain in Hypnotism, and the nature of Clairvoyance." It contains no proof of clairvoyance, beyond the author's statement of its occurrence. Such sentences as "informs me that all the area of the brain in front of the ascending frontal convolution is superficially awake in fairly deep hypnosis," "Touch centres very active (can feel pores on my skin)," and his subjects' description of the "second self" as a "blue cloud" or "a cloud of dark smoke" which "he can send anywhere he pleases when in a hypnotic sleep" will suffice to indicate the general nature of this paper.

Man, Past and Present. By A. H. KEANE. Revised and largely re-written by A. HINGSTON QUIGGIN and A. C. HADDON. Cambridge: University Press. 1920. pp. xi + 582. 36*s*. net.

Mrs Quiggin and Dr Haddon deserve the thanks of all interested in anthropology for bringing this well-known work up to date. Certain sections have had to be entirely rewritten and in many places pages have been suppressed to make room for important information. Full references are given in footnotes to original papers. Sixteen plates of racial types are placed at the end of the book. The volume will rank as a standard text-book on physical and social anthropology. One on the psychological aspect of the subject has still to be written.

Die Ursprünge der Metapher. By HEINZ WERNER. Leipzig: W. Englemann. 1919. pp. 238.

This monograph on the origins of the metaphor forms the third volume of the series *Arbeiten zur Entwicklungspsychologie* edited by Professor Felix Krueger of the University of Leipzig. As the author points out in the preface it is an enlarged section of a work of his, as yet unpublished, on the origins of lyrical poetry.

In the introductory chapter the author formulates the conception of genuine metaphor as the paraphrase of an experience, sentiment or object in terms of another with the characteristic feature of a consciousness on the part of the speaker of the duality of the versions, and of the awareness that the paraphrase is not a real equivalent of or effective substitute for the original.

Genetically the genuine metaphor is preceded, according to the author, by certain forms of "pseudo-metaphors," arising among primitives either from their inability to abstract, or the limitations of their power of expression, both of which lead to paraphrases of things, acts and experiences. Neither, however, is a form of genuine metaphor as neither achieves the duality and conscious distinction between original and paraphrase but both on the contrary are intended to be actually equivalent and even identical. With the anthropomorphic stage of culture and the appearance of magic a complete readaptation of the psychic, especially emotional, life sets in, which for the first time provides the psychological basis for an attitude capable of the dualism requisite for the development of metaphorical speech. This dualism reaches its fullest growth in the cultures which stand under the rule of some tabu. Indeed the whole tabu-adaptation, with its fears, avoidances and repressions and the consequent tendency to lies, fictions, allusion and illusions, is essentially the soil in which metaphor flourishes. So far from being, as might naïvely be supposed, an aid to clearness, explanation and amplification, the metaphor appears as a means of obscuring and hiding the real meaning of the speaker, allowing it to be understood only by the initiated or by the victim against whom he directs his words. As the ideal form of such partial concealment and partial revelation of the meaning, the author pursues the metaphor with a great wealth of illustration and of psychological distinctions through all its various stages from the tabu of names with the complete avoidance of their use, to the alteration of names by additions, by the metathesis of syllables, by alliteration and rhyme, the complete substitution of names and the development of whole tabu-languages; to the phrase metaphors; to the use of metaphors in threats and the transition from scorn to irony and from irony to flattery. In order to prove this, his main thesis, that metaphor has sprung originally from the usages of tabu, the author shows that in primitive poetry the metaphor is characteristic of tabu-cultures and, in his fifth chapter, that the occurrence of metaphorical forms of speech is coincident in its ethnological distribution with the civilisations based on tabu. In chapters 6 and 7 he discusses the various forms of complex metaphors with illustrations from primitive poetry and speech, and various extraneous aids to metaphorical concealment, either linguistic as the use of the question or the play on words, or the more ideal means as allusion, innuendo and pseudo-logic (*Scheinlogik*). His last chapter gives a summary of the genesis of the metaphor and the various paths of its development to the purely poetic and artistic use with which we are familiar in the poetry of civilised races.

The work is one of those monographs which students of the origins of literature, poetry and art will heartily welcome as offering an account of the psychological changes which must be postulated as lying at the back of those forms of aesthetic activity. That some very complex shiftings of motives—the *Motivwandlungen* of the author—must have taken place before anything in the nature of art or poetry in our sense of those terms can have come into existence, has been suspected for some time. Here we find an account, not only amply supported by evidence, but convincing in itself, of these very subtle and very elusive changes and substitutions. It is all the more attractive as an argument of a more general kind, as its material is the apparently so harmless and poetically so respectable practice of metaphorical paraphrase.

Psychoanalysis: a brief account of the Freudian theory. By BARBARA LOW. London: George Allen and Unwin. 1920. pp. 191.

As Dr Ernest Jones states in his preface to this volume, "That the deductions made from psychoanalysis are both novel and not easily acceptable, Miss Low makes plain in her book, and she has not adopted the easier way of concealing these attributes of them. She has chosen the loftier aim of attempting to present all aspects of the psychoanalytical theory fairly and straightforwardly, and yet to bring them within reach of those who have made no previous study of the subject. I can answer for it that she has performed the first part of this task successfully, and can only hope that her readers will find that she has performed the second part with equal success."

The difficulty of achieving this success lies not so much in the arduousness of psychoanalysis as in the fact that it has to be studied from a point of view which is entirely new and quite different from that of every other science. Miss Low therefore wisely begins her book with a chapter on "The Scope and Significance of Psychoanalysis," which clearly explains the psychoanalytic point of view. Then comes a review of "Mental Life" with its unconscious and conscious mechanisms, followed by a discussion of "Repressions," the pleasure-principle and the reality-principle, the egocentric and the social impulses, sublimation and the sexual theory. The remaining chapters deal with dream interpretation, treatment by psychoanalysis and prospective social and educational results.

To those who are unfamiliar with the fact that Freud is not merely the pioneer of psychoanalysis but is also the discoverer of nearly every mental fact and mechanism laid bare by the method, it may become rather tedious to come across Freud's name on practically every page; and many readers may wince at the German tendency to the excessive use of capital letters. Here and there, too, one might find fault with the English; for example, "fantasying." There is no verb 'to fantasy' in English, though there is in German. But these are minor defects. Miss Low's is the best elementary book yet published on psychoanalysis.

An Examination of William James's Philosophy. By J. E. TURNER. Oxford: B. H. Blackwell. 1919. pp. vii + 76. 4*s.* 6*d.* net.

The writer of this brief critical essay confines himself to the consideration of the philosophical theories of Pragmatism and Radical Empiricism which James put forward in the last ten years of his life. He hardly does more than point out the paradoxes, which are and of course were meant to be obvious, although he evidently believes that he has discovered them.

The Field of Philosophy. By Dr JOSEPH A. LEIGHTON. 2nd edition, revised and enlarged. Columbus, Ohio: R. G. Adams and Co. 1919. pp. xii + 485. $2 net.

A small, but comprehensive, text-book for the beginner, written in a clear and straightforward manner.

Treatment of the Neuroses. By Dr Ernest Jones. London: Baillière, Tindall and Cox. 1920. pp. viii + 233. 10*s.* 6*d.* net.

The title of this book does not indicate its full contents. Dr Jones has here given us a valuable account not only of the principal methods of treating the neuroses, but also of the pathology and symptoms of these disorders. The greater part of the book consists of a long chapter devoted to the consideration of hysteria in which "to avoid needless repetition the principles of psychopathology and of mental therapeutics have been dealt with at some length under one heading..., so that the chapters on the other neuroses are correspondingly short." A short chapter on the psychoses and various habits and aberrations concludes the volume.

Throughout, as might be anticipated, the point of view is thoroughly Freudian. The sexual basis of the neuroses is hence brought prominently to the fore. For example, hysteria is attributed to suppressed sexual wishes, neurasthenia (in its narrower, more literal sense) to excessive sexual self-abuse accompanied by intense moral conflict, hypochondria to "excessive erotogenicity of certain areas of the body," alcoholism to repressed homosexuality, anxiety neurosis to "undue sexual excitation...and inadequate relief," anxiety hysteria (e.g. most phobias) to the symbolization of some sexual situation arising in the early development of the psycho-sexual life, obsessional neurosis to buried self-reproach dating from childhood and referring "to certain sexual performances or tendencies," war neurosis generally to "repressed narcissism."

But the truth or error of such pathogeny hardly affects the value of the book. It will for long rank as a standard work upon the various forms, physiological as well as psychological, of treatment of the neuroses. Dr Jones has achieved his aim with surprising impartiality, considering the strong views which he obviously holds on the prepotency of Freudian psychoanalysis. The psychologist may object to his statement that "there is plainly no psychological difference between" suggestion and persuasion, which he bases on the non-psychological ground that "it is obviously a matter of convention or of external and irrelevant circumstances whether a given suggestion is to be considered reasonable or not" (p. 69). Some hypnotists may complain that their special methods of employing hypnosis, not for suggestion but for exploration and subsequent "conscious assimilation of dissociated elements," has not received adequate notice at Dr Jones's hands. Believers in 're-education' methods may resent his conclusion that "probably the greater part of" their "success...is produced by the action of suggestion" (p. 101).

Such criticisms, however, only serve to indicate the extreme interest of the book. It is written in the author's usual clear style and for the most part in non-technical language, so that it serves not only as a compendium of various modes of treatment but also as an excellent introduction for the general public to the psychoanalytic standpoint, written as it is by an ardent Freudian who has had considerable personal experience of most of the therapeutic methods in vogue.

The Foundations of Music. By Dr H. J. Watt. Cambridge: University Press. 1920. pp. xiv + 240. 18*s.* net.

Dr Watt's work is to be welcomed for the guidance which it affords to those who realise that the science of psychology is coming with big strides not to uproot, but to study the rules of harmony. The author is always careful not to detract from what may be learned from the great composers and theorists. In his preface he points out that the foundations of music are not complete until a statistical study of the great composers has been conducted.

After a preliminary survey covering the ground of his earlier book, Dr Watt discusses consonance and dissonance of tones and their fusion. Next, the consonance and dissonance of successive tones is dealt with, and interesting examples are quoted from Schubert's songs which support his statement that smallness of interval is a factor favouring continuity or melodic progression. Beethoven's works might have also been cited, for in them countless passages of successive scale notes are to be found: particularly may be noted the upward scale in the Scherzo of the B♭ Pianoforte Trio, and the downward scale in the Scherzo of the *Eroica* Symphony.

In his discussion of consecutive fifths, it is pleasant to notice that with this as with other harmonic questions Dr Watt deals broadly, even humanely. His psychological sense has saved him from regarding this sequence as necessarily bad. He writes: "The pleasantness of fifths in a certain setting by no means discredits their prohibition under most circumstances. This could be gainsaid only by the pedant who lives on rules and does not apprehend the structures he studies in their primary aspect—aesthetically—at all. But the sole standard of art is the beauty inherent in the created object." In his analysis of this sequence, after pointing out that the prohibition appeared early in the history of the art and much earlier than the formulation of the connexions of inversions, Dr Watt appends the following as the chief theories of prohibition: habit and tradition; excessive sweetness; want of variety; ambiguity of key or tonality; want of relationship; the nature of the interval itself; want of balance. Of these the author appears to favour the last but one.

From a table of progressions prepared by Dr Watt, he says that the following conclusion may be drawn: "If due consideration is given to the prominence of the pair of voices that bear the interval in question, it appears that the immediate repetition of an interval in the same voices is the more offensive the greater the consonance and dissonance of that interval. The point of minimal unpleasantness or of maximal pleasantness (as the case may be) in the series from greatest consonance to greatest dissonance lies amongst the thirds and sixths. These intervals may, therefore, be held to be fusionally neutral." This is interesting when we recall the frequent use of progressions in thirds and sixths to be found in Brahms's music.

Subsequent chapters deal with hidden octaves and fifths, common chords, and melodic motion in relation to degrees of consonance. Independently of Gaudentius, Dr Watt arrives at apparently similar ideas of the properties of 'paraphony.' "In symphony the tones of an interval tend to become indistinguishable through too much unitariness or fusion; in diaphony they sound through or against one another; in paraphony there is balance, so that melodies formed of such intervals will flow evenly side by side, the one not inhibiting the apprehension of the other."

Dr Watt may deservedly lay claim to being a pioneer. Differences of opinion may be held here and there from his views or deductions, but appreciation must be rendered to his thoughtful studies and to the labour he has devoted to them.

Introductory Psychology for Teachers. By Professor EDWARD K. STRONG, jun. Baltimore: Warwick and York. 1920. pp. xii + 233. $1.60.

This book consists of forty-four "lessons" intended to interest school teachers in psychology considered as the science of behaviour. It is an unconventional work, in which a great deal of material, some of it good and much of it bad, is put together in rather a happy-go-lucky manner. As a basis for lectures it might be found helpful.

Graphology and the Psychology of Handwriting. By Professor JANE E. DOWNEY. Baltimore: Warwick and York. 1919. pp. 142. $1.60.

This is a short but serious study of the possibility of using handwriting tests for purposes of mental diagnosis. The author finds that variability in size, slant, alignment and other graphic characteristics may probably afford some indication of typical ways of controlling motor impulses. She candidly states that: "Chiefly, our results are of value in that they outline a programme for further investigation," and it must be admitted that she makes out a very good case for such additional research.

Human Psychology. By Professor HOWARD C. WARREN. New York: Houghton Mifflin Company. 1919. pp. xx + 460.

Prof. Warren adopts the view that "psychology is the science which deals with the mutual interrelation between an organism and its environment," and discusses an enormous variety of types of interrelation from the simplest nerve muscle response to the complications of 'conscious purpose.' In spite of the fact that it is somewhat overloaded with detail, the volume is interesting, and will doubtless prove of use to many students who are beginning the subject.

The Class-Room Republic. By E. A. Craddoch. London: A. and C. Black. 1920. pp. iv + 80. 2*s.* 6*d.* net.

The writer is a form master in a London polytechnic secondary school. He renounces all his offices save that of instructor, and asks his class to form a committee which shall be empowered to punish and to reward, shall have full control of discipline both inside and outside the class room, and shall be responsible not to him, but to the class itself. "For two years," says the writer, "I have not punished a single boy in any way in any of the forms in which I have established, for the purposes of my daily lessons, a class republic. I leave everything except my actual teaching in the hands of the class, with perfect confidence that all will go well. Think of the joys of teaching when all the sordid work of the executioner is left out!"

His next step is to divide his class into two, and to pit one half against the other, successfully reproducing in the class room the atmosphere of the playing-fields. "Nothing in the whole of my professional career," writes this enthusiastic teacher, "has been so fruitful in joyous surprise as the immediate success of this step that I took with much diffidence and doubt." The book is a fascinating little study of the effects of liberty and responsibility, when discreetly controlled by an able teacher.

The Psychology of Musical Talent. By Professor C. E. Seashore. Boston: Silver, Burdett and Co. 1919. pp. xvi + 288.

This is a popular book compiled for the benefit of the music teacher, in which articles dealing with the writer's application of psychological tests to musical ability are collected. There is, in consequence, a good deal of needless repetition and of elementary explanations which are apt to irritate the musical psychologist. The author's main objects are to show the various capacities which go to make up 'musical talent,' to indicate how they may each be tested, and to insist that the musical curriculum should be flexible, the type of training being adapted to the child's individuality. He points out the value of certain tests as drill exercises, and as a means of encouraging musical activities and of obtaining more recruits for school choruses, orchestras and glee clubs. The six principal tests he advocates are those for pitch, loudness, time, consonance, memory and imagery, all save the last of which he has standardised in the form of phonographic records. These tests each take half-an-hour to perform.

The main defect of the book is that it fails to show what is the relation of successes in these (and others of his) tests to musical ability. A promising field of investigation is here open. To know the value of a test in applied psychology, it must be tried on those endowed with the greatest and with the least musical talent. Until this has been done, and until any exceptional results have been analysed and explained, it would seem useless to encourage or to discourage musical education merely on the basis of success at such tests. It is doubtful whether artistic genius will ever be hidden or revealed owing to lack or zeal of testing; in this connexion we may note that the writer lays little or no stress on musical inventiveness in his scheme of tests. His complete scheme includes tests of auditory and motor imagery, grip, ergography, precision of movement, motor reliability, simple, complex and choice reactions, rhythmic action, time-keeping, voice register, quality of voice, etc.; but we have yet to know the relative importance of these tests for different musical requirements and ends. As a pioneer worker, Dr Seashore is to be commended for having with such patience and enthusiasm essayed to lay the foundations of vocational psychology applied to art.

Functional Nerve Disease: an epitome of War Experience for the Practitioner. Edited by Dr H. Crichton Miller. London: Frowde, Hodder and Stoughton, Ltd. 1920. pp. xi + 208. 8*s.* 6*d.* net.

This book is a collection of papers by various writers, many of them well-known psychologists. Its object, as explained in the preface, is to present to the medical practitioner a simple and concise picture of the functional neuroses of war-time. Each writer takes a special part of the subject, and deals with it entirely in his own fashion, apparently without correspondence with the writer of any other chapter. The inevitable consequence of such an arrangement is to bring out most prominently the wide differences of view which exist among individual psychologists at the present day, and to demonstrate more effectively than anything else could do the sharp divisions of opinion that occur between the different schools of psychological thought. Dr McDougall, in a summary which is placed at the end of the book, admits this want of co-ordination between the different writers, but he has summarised most skilfully the points upon which they agree. That such a chapter should be necessary is sufficient comment upon the difficulty that is presented by this method of preparing a book on Psychological Medicine. The reader would be well advised to treat the book as a collection of separate papers, and not to attempt to read it straight through. The abrupt changes of style and language between the different authors makes the confusion that exists in their terminology more noticeable. Most of the authors appear to accept the Freudian theory of the causation of the psycho-neuroses, but they have not been content to accept, at the same time, the Freudian nomenclature. For example Dr Rivers introduces the word 'suppression' as being different from the word 'repression' which hitherto has included the process which he describes.

In the first chapter upon Physical Aetiology, Dr Crichton Miller brings to our notice the possibility of agreement between the materialist and the psycho-therapist. The value of this is instantly apparent, and the clear way in which Dr Miller explains the case from both points of view should make the co-ordination between the two types of practitioner much more simple than it has hitherto been.

The chapter on Differential Diagnosis by Dr George Riddoch is one of the most valuable contributions to the same problem of co-ordination that have been published of recent years.

Both in this chapter, and in a later chapter upon Regression by Dr Maurice Nicoll, the theory of Hughlings Jackson is endorsed. We have here a timely reminder of a theory which both neurologists and psychologists are constantly in danger of forgetting.

Perhaps the most interesting chapters to the practitioner are those on Regression, The Mother Complex, and Psycho-analysis. These contain very much that is of value to those who would carry out psychological investigation, and if there is anyone nowadays who doubts the advisability of analysis in cases of psycho-neuroses, he should read these exceptionally sane contributions to the subject.

Some disappointment may be felt when it is found that in the chapter on Psycho-analysis no definite formula is given for the cure of this or that particular form of neurosis, but it should be more widely recognised that psycho-analysis cannot be taught from the book, but only by the practitioner submitting himself or his patient to analysis, finding out from errors where the truth lies, and working out a personal method of his own.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

General Meetings.

March 13, 1920.	Human Motives in the Light of Recent Discussion, by W. McDougall. A Linguistic Factor in English Characterology, by Ernest Jones.
May 8, 1920.	Speech Inscriptions in Normal and Abnormal Conditions, by E. W. Scripture. Camouflage in Land Warfare, by Adrian Klein.

Sectional Meetings.

(a) *Education Section.*

February 12, 1920.	The Dreams of Children in Blind, Deaf and Industrial Schools, by C. W. Kimmins.
March 9, 1920.	A Statistical Survey of Arithmetical Ability, by D. J. Collar.
April 14, 1920.	The Purposeful Act as the Unit in the Educative Process, by W. H. Kilpatrick.
May 12 & 26, 1920.	Left-handedness and Mental Deficiency, by Hugh Gordon (Joint Meeting with Medical Section).

(b) *Industrial Section.*

February 25, 1920.	The Effect of Change in Hours of Work on Output, by H. M. Vernon.
March 25, 1920.	Some of the Psychological Causes of Labour Turn-over in Factories, by G. M. Broughton.

(c) *Medical Section.*

February 18, 1920.	The Revival of Emotional Memories and its Therapeutic Value, by W. Brown, C. S. Myers and W. McDougall.
April 28, 1920.	Psychological Adaptation, by Constance Long.
May 12 & 26, 1920.	Left-handedness and Mental Deficiency, by Hugh Gordon (Joint Meeting with Education Section).

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